19.452/B

LANSDOWNE
HOUSE

MEDICAL REPORTS,

ON THE

EFFECTS OF WATER,

COLD AND WARM,

AS A REMEDY IN

FEVER, AND FEBRILE DISEASES;

WHETHER APPLIED TO THE SURFACE OF THE BODY, OR
USED AS A DRINK:

WITH

OBSERVATIONS ON THE NATURE OF FEVER;

AND ON THE EFFECTS OF

OPIUM, ALCOHOL, AND INANITION.

BY JAMES CURRIE, M. D. F. R. S.

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> *Intentiones operationum, quas proposuimus (ut arbitramur) verissimae sunt, remedia intentionibus fida.* * * *Rem ipsam experimentum et comprobavit et promovebit.* * * *Opera consilii cujusque prudentioris, sunt effectu admiranda, ordine quoque egregia, modis faciendi tanquam vulgaria.*
>
> BACON. Historia vitae et mortis.

LIVERPOOL,
PRINTED BY J. M'CREERY,
FOR CADELL AND DAVIES, LONDON.
1797.

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TO THE

RIGHT HON. SIR JOSEPH BANKS,

Baronet and Knight of the Bath, President of the Royal Society, &c. &c. &c.

SIR,

IN presenting this volume to you, I beg leave to say a few words on the views with which it was written, and the circumstances that gave rise to the publication.

About eighteen years ago, when I was at Edinburgh, it fell to my lot to write a paper on the influence of cold on the living body, for one of the societies of students, of which I was a member. In defending my speculations against some ingenious opponents, a perpetual contradiction occurred as to facts, which a reference to original authorities, did not enable me to remove; for I discovered, that the accounts given of the temperature of the system under disease, even by the most approved authors, are, with a few exceptions, founded, not on any exact measurement of heat, but on the sensations of the patient himself, or his attendants.

Impressed with the belief, that till more accurate information is obtained respecting the actual temperature in different circumstances of health, and disease, no permanent theory of vital motion can be established, nor any certain progress made in the treatment of those diseases in which the temperature is diminished or increased, I have occasionally since that time, observed and recorded such facts as related to the subject; intending, one day or other, to lay my observations before the public, if they acquired an importance that deserved attention. In the outset of this undertaking, nothing seemed wanting but accurate thermometers, and a moderate portion of time and attention; and I embraced in imagination the whole effects of temperature upon health and disease; a range of inquiry which experience has convinced me it would be temerity and folly to hope to go through. In this general view of the subject, some valuable communications have however been made to me by my much respected friend Dr. Percival; which do not apply to the particulars treated of in the following volume, but which I am not without the hopes of specifying at some future period, when I may have occasion to employ them.

Though I have some time foreseen, that the delay of publishing till my original plan was executed, was likely to render my labours wholly abortive, or to convert the imperfect product from a gift into a legacy; yet I should not have given to the world so detached and unfinished a work as the present, according to the views I had originally entertained, but for the circumstances I am about to relate.

By the accounts received at the beginning of the present year, the fever of the West Indies appeared to continue its desolating progress with little abatement, and in America to be beginning its ravages anew.—With this pestilence, science seemed hitherto to have contended, in a great measure, in vain, and new methods of opposing it, were not merely justifiable, but requisite. At this time an account of the success of the nitric acid in Lues and Hepatitis, as employed by your correspondent Mr. Scott, of Bengal, was through your means given to the public. His theory, suggested by the new chemistry, did not appear promising; but it was neither wise nor candid to reject, on that account, his experience. The first trials which I made of his practice persuaded me, that though the success of the nitric acid might involuntarily have been somewhat exaggerated by a warm imagination and a benevolent heart, it assuredly did succeed in certain cases of the diseases in question, and that a remedy of *great power*, and *of perfect safety*, was introduced into medicine. These opinions subsequent experience has fully established in my mind. In the fever of the West Indies mercury had been much employed, and though different notions were entertained of its mode of operation, it seemed on the whole the most approved remedy. Since the nitric acid appeared to be a substitute for mercury in other cases, it seemed reasonable to try its effects in this fatal fever. I suggested this practice by letter to some practitioners in the West Indies, and I submitted my notions on the subject to you; sensible that your character and station might bring that into immediate notice, which the influence of a private individual could but slowly effect.—Whatever difference there might be in some of our opinions, I was confident there was none in our wishes for alleviating human misery, and mitigating the destruction of this desolating war. Your conduct even exceeded my expectation.

Our correspondence on this occasion turned my views to the other means of opposing this pestilence. Ablution with cold water in fever had been so long employed at the hospital here, and in private practice, by my friends and colleagues Dr. Brandreth and Dr. Gerard, as well as myself, that it was become general in Liverpool, and common in the county of Lancaster. So long ago as the year 1791, a general statement by Dr. Brandreth of its advantages had been published by Dr. Duncan, in the Medical Commentaries of that year. It had also been repeatedly mentioned by me in private correspondence; it had often been recommended to the surgeons of African ships in those examinations required by the legislature, and which are chiefly made by the physicians and surgeons of our hospital. On different occasions likewise I had not only explained, but exhibited the practice, to practitioners from a distance, and particularly to one or two going to the West Indies. A method of treatment so bold and so contrary to common prejudices, made however, as it appears, slow progress. The mode of operation of our remedy has been misapprehended; the proper period for using it has not been understood; and on some occasions having been resorted to improperly, the consequences have brought it into disrepute. Reflecting on these circumstances, and exposed by situation to the re-iterated sounds of death from the Western world, my decision was speedily made. I resolved no longer to delay an account of our treatment of fever, in the expectation of including it in a larger field of discussion, or in a form more conducive to reputation; and the fruit of this determination is the work now presented to you. In treating my subjects, perspicuity has been studied rather than rigorous method; I have every where endeavoured to make my steps so plain, that they may be distinctly traced; the most important points are impressed again and again to guard against mistake; my thermometrical observations have enabled me to give a precision to the directions for the use of the affusion of cold water which otherwise they could not have had; and, if I do not flatter myself, have laid a foundation for my reasonings, which speculations on fever have seldom possessed. I have guarded against the unnecesary use of technical as well as of general expressions. It were better perhaps that medicine, like other branches of natural knowledge, were brought from its hiding-place, and exhibited in the simplicity of science and the nakedness of truth. If it had been in my choice, I would not have adopted the language of theory, like Boerhaave, or Sydenham; but have exhibited a medical work in the phraseology that Bacon, had he lived in our days, would have used.

Possibly this notice may procure me some readers among men of general science; and this I confess to be one of my objects in dedicating the work to you. It is naturally an author's wish that his book may be read by those who can appreciate it, and who from their situations may have it in their power to bring its precepts into practice. In both these points of view I appeal to you—to your scientific knowledge, and to your generous heart. The work that I address to you, is in a great measure practical. A man of genius, at the head of a fleet or army, would probably find little difficulty in understanding it; and possibly, if he understood it, there might be occasions on which it would afford scope to his humanity and patriotism. But whatever be its fate with men professedly military, I trust it will not be overlooked by the medical practitioners of our fleets and armies; a most meritorious class of the profession, to whom a great part of the improvements in the modern practice of medicine are to be ascribed.

Conceiving that the circulation of this volume, as well as its usefulness, might be extended by connecting the history of the affusion of cold water in fever, with other views of the same remedy, and with a few observation on the other remedies in fever, I have entered into these points, without any very strict regard to method; and have been insensibly led to speak of some of the operations of temperature on the body in health, a subject which I had reserved. Such as it is, this volume may serve as the first of a series on similar subjects, if I should ever write them; and it may, I hope, stand alone, if I should write no more.

I am sensible that some of these particulars would have appeared with more propriety in a professed preface; but having entered on certain explanations in my address to you, I have concluded these preliminary observations in the same form; a freedom that I trust you will forgive.

I cannot conclude without declaring the sense I entertain of your candour and politeness. Accept the tribute of my respect.—May you live long to cultivate and protect the sciences—the sciences, whose utility is beyond dispute; whose progress is superior to obstruction; and which, of all the possessions of man, seem least to partake of the imperfection of his nature!

I have the honour to be,
Sir,
Your faithful
And very obedient Servant,
JAMES CURRIE.

Liverpool, 31*ſt Oct.* 1797.

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ERRATA.

Page 10,	line 6,	*dele* the comma.	
18,	13,	*for* illuſtriate	*read* illuſtrate.
34,	5,	ſtraight	ſtreight.
63,	6,	*dele* as well.	
78,	18,	*for* uſed	drank.
139,	2,	facts	points.
140,	23,	ſet	ſets.
141,	2,	ſet	ſets.
230,	8,	was	were.
240,	18,	56th	5-6ths.

Appendix, page 16, line next the bottom, *for* concluſive *read* convulſive.

Dr. Ferriar corrects the information in page 217, laſt ſentence of the note, thus; "from the 4th of May, to the 7th of October, 1797, incluſive, "149 patients have been admitted into the Houſe of Recovery under fever, "*four* of whom have died."

CHAP. I.

Narrative of Dr. Wright.

IN the London medical journal for the year 1786, Dr. William Wright,* formerly of the iſland of Jamaica, gave an account of the ſucceſsful treatment of ſome caſes of fever by the ablution of the patient with cold water.

" On the 1ſt of Auguſt, 1777," ſays Dr. Wright, " I embarked in a ſhip bound to Liverpool, and ſailed the ſame evening from Montego Bay. The maſter told me he had hired ſeveral ſailors on the ſame day we took our departure; one of whom had been at ſick quarters on ſhore, and was now but in a convaleſcent ſtate. On the 23d of Auguſt, we were in the latitude of Bermudas, and had had a very heavy gale of wind for three days, when the above-mentioned man relapſed,

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* Now I believe phyſician to the army in the Weſt Indies

and had a fever with ſymptoms of the greateſt malignity. I attended this perſon often, but could not prevail with him to be removed from a dark and confined ſituation to a more airy and convenient part of the ſhip; and as he refuſed medicines, and even food, he died on the eighth day of his illneſs.

" By my attention to the ſick man I caught the contagion, and began to be indiſpoſed on the 5th of September, and the following is a narrative of my caſe, extracted from notes daily marked down: I had been many years in Jamaica, but, except being ſomewhat relaxed by the climate, and fatigue of buſineſs, I ailed nothing when I embarked. This circumſtance, however, might perhaps diſpoſe me more readily to receive the infection.

" Sept. 5th, 6th, 7th, ſmall rigors now and then—a preternatural heat of the ſkin—a dull pain in the forehead—the pulſe ſmall and quick—a loſs of appetite, but no ſickneſs at ſtomach—the tongue white and ſlimy—little or no thirſt—the belly regular—the urine pale and rather ſcanty—in the night reſtleſs, with ſtarting and delirium.

" Sept. 8th, every ſymptom aggravated, with pains in the loins and lower limbs, and ſtiffneſs in the thighs and hams.

" I took a gentle vomit in the ſecond day of this illneſs, and next morning a decoction of tamarinds; at bed-time, an opiate, joined with antimonial wine, but this did not procure ſleep, or open the pores of the ſkin. No inflammatory ſymptoms being preſent, a drachm of Peruvian bark was taken every hour for ſix hours ſucceſſively, and now and then a glaſs of port wine, but with no apparent benefit. When upon deck, my pains were greatly mitigated, and the colder the air the better. This circumſtance, and the failure of every means I had tried, encouraged me to put in practice on myſelf what I had often wiſhed to try on others, in fevers ſimilar to my own.

" Sept. 9th, having given the neceſſary directions, about three o'clock in the afternoon I ſtripped off all my cloaths, and threw a ſea-cloak looſely about me till I got upon the deck, when the cloak alſo was laid aſide: Three buckets full of ſalt water were then thrown at once on me, the ſhock was great, but I felt immediate relief. The head-ach and other pains inſtantly abated, and a fine glow and diaphoreſis ſucceeded. Towards evening, however, the febrile ſymptoms threatened a return, and I had again recourſe to the ſame method as before, with the ſame good effect. I now took food with an appetite, and for the firſt time had a ſound night's reſt.

"Sept. 10th, no fever, but a little uneaſineſs in the hams and thighs—uſed the cold bath twice.

"Sept. 11th, every ſymptom vaniſhed, but to prevent a relapſe, I uſed the cold bath twice.

"Mr. Thomas Kirk, a young gentleman, paſſenger in the ſame ſhip, fell ſick of a fever on the 9th of Auguſt. His ſymptoms were nearly ſimilar to mine, and having taken ſome medicines without experiencing relief, he was deſirous of trying the cold-bath, which with my approbation, he did on the 11th and 12th of September, and, by this method was happily reſtored to health. He lives at this time (Jan. 1786) near Liverpool."

To this intereſting narrative, Dr. Wright adds ſome general obſervations on the traces that are to be found of the uſe of cold water internally and externally in fevers, in ſeveral works ancient and modern. But whether he himſelf purſued this practice any farther, I have not been informed.

Having before experienced that Dr. Wright was a ſafe guide,* I immediately on reading this narrative determined on following his practice in the preſent inſtance; and before an opportunity occurred of carrying my intention into effect, I was farther encouraged, by learning that my reſpectable colleague Dr. Brandreth, had employed cold water externally in ſome recent caſes of fever with happy effects.

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* See a paper in the memoirs of the London Medical Society, Vol. III. p. 147. to be found in a ſubſequent part of this Volume.

CHAP. II.

Hiſtory of a fever which broke out in the Liverpool Infirmary.

IN the 9th of Dec. 1787, a contagious fever made its appearance in the Liverpool Infirmary. For ſome time previouſly the weather had been extremely cold, and the diſcipline of the houſe, owing to cauſes which it is unneceſſary to mention, had been much relaxed. The intenſity of the cold prevented the neceſſary degree of ventilation, and the regulations for the preſervation of cleanlineſs had been in ſome meaſure neglected. Theſe circumſtances operated particularly on one of the wards of the eaſtern wing, employed as a lock-hoſpital for females, where the contagion firſt appeared. The ſever ſpread rapidly, and before its progreſs could be arreſted, ſixteen perſons were affected, of which two died.—Of theſe ſixteen, eight were under my care. On this occaſion I uſed for the firſt time the affuſion of cold water, in the manner deſcribed by Dr. Wright. It was firſt tried in two caſes only, the one in the ſecond, the other in the fourth day of fever. The effects correſponded exactly with thoſe mentioned by him to have occurred in his own caſe, and thus encouraged, the remedy was employed in five other caſes. It was repeated daily, and of theſe ſeven patients, the whole recovered. In the eighth caſe, the aſperſion of cold water ſeemed too hazardous a practice, and it was not employed. The ſtrength of the patient was much impaired by lues, and at the time of catching the contagion, ſhe laboured under ptyaliſm. I was not then aware that this laſt circumſtance, formed no objection againſt the cold affuſion, and in a ſituation ſo critical, it was thought imprudent to uſe it. The uſual remedies were directed for this patient, particularly, bark, wine, and opium, but unſucceſsfully; ſhe died on the 16th day of her diſeaſe.

From this time forth, I have conſtantly wiſhed to employ the affuſion of cold water in every caſe of the low contagious fever, in which the ſtrength was not already much exhauſted; and I have preſerved a regiſter of a hundred and fifty three caſes, in which the cure was chiefly truſted to this remedy. Of theſe, ninety-four occurred in the hoſpital in the four years ſubſequent to the period already mentioned, twenty-ſeven in private practice, and thirty-two in the 30th regiment of foot, when quartered in Liverpool in the year 1792. Of late I have not thought it neceſſary to regiſter all the caſes in which this remedy has been employed. Having ſatisfied myſelf of its extraordinary efficacy, and of the precautions neceſſary in uſing it, I have found it the ſhorter method as well as the more inſtructive, to record the inſtances in which it has proved unſucceſsful. To relate the whole of my experience would be a tedious and an uſeleſs labour. I purpoſe to digeſt the reſults under a few diſtinct heads, ſupporting and illuſtrating each general propoſition by an ample detail of caſes. Before, however, this preliminary account is cloſed, it will be uſeful to enter more particularly into the hiſtory of the contagious fever which broke out in the 30th regiment, becauſe the account of its riſe, progreſs, treatment, and termination, will ſupport in a ſtriking manner the doctrines I wiſh to eſtabliſh, and if I do not greatly deceive myſelf, may afford important inſtruction, as well as encouragement, to thoſe whoſe duty may call them to oppoſe the progreſs of contagious fever in ſimilar ſituations.

CHAP. III.

Hiſtory of a Fever which occurred in the 30th Regiment.

THE 30th Regiment, as is uſual with troops in Liverpool, was billetted in the town, but paraded and mounted guard in the fort, ſituated north of the town, and on the banks of the river. The general guard room had been uſed previous to the arrival of the 30th, as a place of confinement for deſerters; it was extremely cloſe and dirty, and under it was a cellar, which in the winter had been full of water. This water was now half evaporated, and from the ſurface iſſued offenſive exhalations.

In a dark, narrow, and unventilated cell, off the guard-room, it was uſual to confine ſuch men as were ſent to the guard for miſbehaviour, and about the beginning of June, (1792) ſeveral men had been ſhut up in this place on account of drunkenneſs, and ſuffered to remain there twenty-four hours, under the debility that ſucceeds intoxication. The typhus, or jail fever, made its appearance in two of theſe men about the firſt of the month, and ſpread with great rapidity. Ten of the ſoldiers labouring under this epidemic, were received into the Liverpool Infirmary, and the wards allotted to fever could admit no more. The contagion continuing its progreſs, a temporary hoſpital was fitted up at the fort, and I was requeſted to give my aſſiſtance there to the ſurgeon of the regiment, by Captains Brereton* and Torriano.†

In two low rooms, each about fifteen feet ſquare, were fourteen patients labouring under fever. They were in different ſtages of its progreſs: one was in the fourteenth day of the diſeaſe, two were in the twelfth, and the reſt from the ninth to the fourth incluſive. The ſymptoms of the fever were very uniform. In every caſe there was more or leſs cough, with mucous expectoration: in all thoſe who had ſuſtained the diſeaſe eight days and upwards, there were petechiæ on the ſkin; in ſeveral there were occaſional bleedings from the noſtrils, and ſtreaks of blood in the expectoration. The debility was conſiderable from the firſt, and this had been increaſed in ſeveral caſes by the uſe of veneſection, before the nature of the epidemic was underſtood.

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* Now Colonel Brereton of the 63d foot.

† The gallant and accompliſhed officer who fell ſoon after at the heights of Pharon, in the defence of Toulon.

underſtood. The pulſe varied from 130 ſtrokes in the minute to 100; the heat roſe in one caſe to 105° of Fahrenheit, but was in general from 101° to 103°; and towards the latter ſtages of the diſeaſe it was ſcarcely above the temperature of health. Great pain in the head, with ſtupor pervaded the whole, and in ſeveral inſtances there occurred a conſiderable degree of the low delirium.

Our firſt care was to ventilate and clean the rooms, which were in a high degree foul and peſtilential. Our ſecond was to waſh and clean the patients themſelves. This was done by pouring ſea-water, in the manner already deſcribed, over the naked bodies of thoſe whoſe ſtrength was not greatly reduced, and *whoſe heat was ſteadily above the temperature of health.* In thoſe advanced in the fever, whoſe debility was of courſe great, we did not venture on this treatment, but contented ourſelves with ſpunging the whole ſurface of the body with tepid vinegar, a practice that in every ſtage of fever is ſalutary and refreſhing.

Our next care was to ſtop the progreſs of the infection. With this view, the guard-houſe was at firſt attempted to be purified by waſhing and ventilation, the greateſt part of its furniture having been burnt or thrown into the ſea. All our precautions and exertions of this kind were however found to be ineffectual. The weather was at this time wet, and extremely cold for the ſeaſon; the men on guard could not be prevailed on to remain in the open air; and from paſſing the night in the infected guard-room, ſeveral of the privates of the ſucceſſive reliefs on the 10th, 11th, and 12th of the month, caught the infection.—In ſeveral of theſe the fever ran through its courſe; and in others, it was immediately arreſted by the affuſion of ſea-water as already deſcribed. No means having been found effectual for the purification of the guard-room, it was ſhut up, and a temporary ſhed erected in its ſtead.—Still the contagion proceeded; on the morning of the 13th three more having been added to the liſt of the infected.—On that day, therefore, the whole regiment was drawn up at my requeſt, and the men examined in their ranks: Seventeen were found with ſymptoms of fever upon them.—It was not difficult to diſtinguiſh them as they ſtood by their fellows.—Their countenances were languid, their whole appearance dejected, and the adnata of their eyes had a dull red ſuffuſion. Theſe men were carefully ſeparated from the reſt of the corps, and immediately ſubjected to the cold affuſion, always repeated once, and ſometimes twice a day. In fifteen of the number the contagion was extinguiſhed; but two went through the regular diſeaſe. On the ſame day, the commanding officer, at my deſire, iſſued an order for the whole of the remaining part of the regiment to bathe in the ſea; and for ſome time they were regularly muſtered, and marched down at high water, to plunge into the tide.

Theſe means were ſucceſsful in arreſting the epidemic: after the 13th of June no perſon was attacked by it. It extended to fifty eight perſons in all, of which thirty-two went through the regular courſe of the fever, and in twenty-ſix the diſeaſe ſeemed to be cut ſhort by the cold affuſion. Of thirty-two already mentioned, two died. Both of theſe were men whoſe conſtitutions were weakened by the climate of the Weſt Indies; both of them had been bled in the early ſtages of the fever; and the one of them being in the 12th, the other in the 14th day of the diſeaſe when I firſt viſited them, neither of them was ſubjected to the cold affuſion. The water employed on this occaſion was taken up from the river Merſey cloſe by the fort. It was at that time of a temperature from 58° to 60° of Fahrenheit, and it contains in ſolution from a 32d to a 33d part of ſea-ſalt.

In hoſpitals, manufactories, and priſons—ſituations in which the low contagious fever ſo frequently originates, the practice I have detailed may be followed with great eaſe, ſafety, and advantage; but it is in a more particular degree applicable to this contagion when it appears on ſhip-board, becauſe in that ſituation the uſual means of prevention or cure are neceſſarily limited, and the imminence of the danger requires a remedy that operates with ſpeed as well as efficacy. The waters of the ocean afford this remedy; in every point of view a moſt happy one for mariners, ſince it can be applied almoſt as eaſily as it can be procured.

CHAP. IV.

The manner in which the affuſion of cold water ought to be uſed in Fever.

HAVING given this general account of my experience of this remedy in fever, it will now be neceſſary to enter more particularly on the rules which ought to govern its application, and on the different effects to be expected from it, according to the different ſtages of the diſeaſe in which it is employed. It will be proper to premiſe, that when the term fever is uſed in the preſent work without any adjunctive, it is the low contagious fever that is meant. This is the Typhus of Dr. Cullen; the contagious fever of Dr. Lind; the Typhus irritativus of Dr. Darwin. In popular language, it is generally called the nervous fever, and where particular ſymptoms appear, the putrid fever. It is generally produced in ſituations where there is a want of cleanlineſs, and more eſpecially of ventilation; and when produced, it is propagated by contagion. This is the common fever of England; its ſymptoms have been detailed with great minuteneſs in a variety of modern publications, and I have therefore declined repeating deſcriptions that are every where to be met with. Dr. Cullen has defined the diſeaſe as follows—"Morbus contagioſus; calor parum auctus; pulſus "parvus, debilis, plerumque frequens; urina parum "mutata; ſenſorii functiones plurimum turbatae; "vires multum imminutae." In ſixteen years practice I have found the contagious fever of Liverpool remarkably uniform, and in general to correſpond exactly with this conciſe and perſpicuous definition. This diſeaſe prevails chiefly among the poor, who from the nature of their diet and habits, are peculiarly expoſed to the cauſes that produce it. Seldom extending itſelf in any conſiderable degree, among the other claſſes of the community, it has been ſuppoſed that Liverpool was little ſubject to fever; but this will be ſhewn, from authentic documents, to be a great and a pernicious error. Let us proceed at preſent to inquire into the rules that ought to govern the uſe of the affuſion or aſperſion of cold water in this diſeaſe.

Whoever has watched the progreſs of fever, muſt have obſerved the juſtice of the obſervation made by Cullen, Vogel, De Haen, and others, that even thoſe genera which are denominated continued, are not ſtrictly ſuch, but have pretty regular and diſtinct exacerbations and remiſſions in each diurnal period. In this ſpace of time, Dr. Cullen contends that an attentive obſerver may commonly diſtinguiſh two ſeparate paroxyſms.* My obſervations do not enable me to confirm his poſition in its full extent—but one exacerbation, and one remiſſion in the twenty-fours, ſeems generally obſervable. The exacerbation uſually occurs in the afternoon, or evening, the remiſſion towards morning. Theſe exacerbations are marked by increaſed fluſhing, thirſt, and reſtleſſneſs. If the heat of the patient be, at ſuch times, taken by the thermometer, it will be found to have riſen one or two degrees in the central parts of the body, above the average heat of the fever, and ſtill more on the extremities.—The ſafeſt and moſt advantageous time for uſing the aſperſion or affuſion of cold water, is when the exacerbation is at its height, or immediately after its declination is begun; and this has led me almoſt always to direct it to be employed from ſix to nine o'clock in the evening; but it may be ſafely uſed at any time of the day, *when there is no ſenſe of chillineſs preſent, when the heat of the ſurface is ſteadily above what is natural, and when there is no general or profuſe perſpiration.*—Theſe particulars are of the utmoſt importance.

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* Paroxyſmis quovis die binis.
Gen. Morbor. vol. ii. *p.* 67.

1. If the aſperſion of cold water on the ſurface of the body be uſed during the cold ſtage of the paroxyſm of fever, the reſpiration is nearly ſuſpended; the pulſe becomes fluttering, feeble, and of an incalculable frequency; the ſurface and extremities become doubly cold and ſhrivelled, and the patient ſeems to ſtruggle with the pangs of inſtant diſſolution. I have no doubt, from what I have obſerved, that in ſuch circumſtances, the repeated affuſion of a few buckets of cold water would extinguiſh life. This remedy ſhould therefore never be uſed when any conſiderable ſenſe of chillineſs is preſent, even though the thermometer, applied to the trunk of the body, ſhould indicate a degree of heat greater than uſual.

2. Neither ought it to be uſed when the heat meaſured by the thermometer is leſs than, or even only equal to the natural heat, though the patient ſhould feel no degree of chillineſs. This is ſometimes the caſe towards the laſt ſtages of fever,

when the powers of life are too weak to ſuſtain or react under, ſo powerful a ſtimulus.

3. It is alſo neceſſary to abſtain from the uſe of this remedy when the body is under profuſe perſpiration, and this caution is more important in proportion to the continuance of this perſpiration. In the commencement of perſpiration, eſpecially if it has been brought on by violent exerciſe, the affuſion of cold water on the naked body, or even immerſion in the cold bath, may be hazarded with little riſque, and ſometimes may be reſorted to with great benefit. After the perſpiration has continued ſome time and flowed freely, eſpecially if the body has remained at reſt, either the affuſion or immerſion are attended with danger, even though the heat of the body at the moment of uſing them be greater than natural.—Perſpiration is always a cooling proceſs in itſelf, but in bed it is often prolonged by artificial means, and the body is prevented from cooling under it to the natural degree, by the load of heated clothes. When the heat has been thus artificially kept up, a practitioner, judging by the information of his thermometer only, may be led into error. In this ſituation, however, I have obſerved that the heat ſinks rapidly on the expoſure of the ſurface of the body even to the external air, and that the application of cold water, either by affuſion or immerſion, is accompanied by a loſs of

heat and a deficiency of re-action, which are altogether inconſiſtent with ſafety.—Each of theſe points will be illuſtrated more fully in the ſequel.

Under theſe reſtrictions the cold affuſion may be uſed at any period of fever; but its effects will be more ſalutary in proportion as it is uſed more early. Its general effects will be more clearly illuſtrated by the following caſes. They are a ſelection from a great number, the records of which have been preſerved, and which lead to the ſame reſults. They are ſo arranged as to exhibit the ſalutary effects of the affuſion of cold water in the different ſtages of fever, and to illuſtriate the precautions laid down againſt uſing it improperly. If they ſhould appear tedious after what has already been mentioned, this muſt be forgiven; on a ſubject ſo important and ſo little underſtood, it is better to incur the charge of tedioufneſs than the hazard of being obſcure.

CHAP. V.

CASE I.

Caſes in which the Affuſion of Cold Water was uſed in the different ſtages of fever.

January 1, 1790.

A Nurſe in the fever ward of the Infirmary having ſeveral patients under her care, caught the infection. She was ſeized with violent rigors, chillineſs, and wandering pains, ſucceeded by great heat, thirſt, and head-ach. Sixteen hours after the firſt attack, her heat at the axilla was 103° of Fah^t. her pulſe 112 in the minute and ſtrong; her thirſt great, her tongue furred, and her ſkin dry.

Five gallons of ſalt water, of the temperature of 44°, were poured over her naked body, and after being haſtily dried with towels, ſhe was replaced in bed: when the agitation and ſobbing had ſubſided, her pulſe was found to beat at the rate of 96 ſtrokes in the minute, and in half an hour afterwards it had fallen to 80. The heat was reduced to 98° by the ablution, and half an hour afterwards it remained ſtationary: The ſenſe of heat and the head-ach were gone, and the thirſt nearly gone. Six hours afterwards ſhe was found perfectly free of fever, but a good deal of debility remained.

Small doſes of colombo were ordered for her with a light nouriſhing diet, and for ſeveral days the cold affuſion was repeated at the ſame hour of the day; the fever never returned.

During the progreſs of fever when epidemic, a great number of caſes ſimilar to the above have occurred, in which the diſeaſe was ſuddenly cut ſhort by the uſe of the cold affuſion on the firſt and ſecond day; twenty ſix of theſe caſes were in the 30th regiment, as has already been ſtated. In ſuch inſtances, the reſult was ſo preciſely ſimilar to what occurred in the caſe I have related, that it would be to no purpoſe to detail them.

When an epidemic fever is ſpreading, and the danger is known, patients will take the alarm on the firſt attack, and the power, as well as the utility of ſuch a remedy as the cold affuſion, in ſuch ſituations of general danger, will be eaſily imagined.—It cannot be employed too ſoon after the firſt attack, provided the original chill is over, and the hot ſtage is firmly eſtabliſhed.

In caſes in which the affuſion was not employed till the third day of fever, I have ſeen ſeveral inſtances of the ſame complete ſolution of the diſeaſe. I have even ſeen this take place, when the remedy has been deferred till the fourth day; but this is not common.—The following caſe will point out the uſual effects of this remedy in the third and fourth days of the diſeaſe.

CASE II.

Jan. 17, 1790. A. B. aged nineteen, a pupil of the Infirmary, caught the infection in attending the fever-ward. When I ſaw him, ſeventy-eight hours had elapſed ſince the firſt attack: he was of courſe in the fourth day of the diſeaſe. He had all the uſual ſymptoms—head-ach, thirſt, furred tongue, pain in the back and loins, with great debility. His heat was 101°, and his pulſe 112 in the minute.

A bucket full of ſalt water was poured over him as uſual at noon on the 17th. His heat ſunk to 99°, and his pulſe to 98 in the minute. A profuſe perſpiration followed, with the ceſſation of all his feveriſh ſymptoms.—This intermiſſion continued for ſeveral hours, during which he enjoyed ſome comfortable ſleep: but at five in the afternoon he was again ſeized with feveriſh rigors, followed by heat, thirſt, and head-ach, as before. An hour afterwards, the hot ſtage was eſtabliſhed; his heat was 100°, his pulſe 100. The ſame quantity of cold water was again poured over him, and with ſimilar effects. His pulſe fell immediately to 80 and became more full, his heat became natural. The following night he took twenty drops of laudanum and ſlept well.

On the 18th at noon his pulſe was 96 and ſoft; his ſkin moiſt, but a little above the natural heat. His tongue was a little furred, and his head ached: he alſo complained of thirſt; the heat at the axilla was 100°.—The ſame remedy was again applied. He was greatly refreſhed by it. The pulſe fell to 90, the ſkin became cool, the thirſt went off, and all the feveriſh ſymptoms vaniſhed.

On the 19th his pulſe was 88, his heat natural, the thirſt and head-ach were gone, and his appetite improving. The ablution was repeated for the laſt time at the ſame hour of the evening.

On the 20th his pulſe was 78 and ſoft, his tongue clean, and his appetite farther improved. He had ſtill ſome remains of debility on the 21ſt, but on the 22d he was free of complaint. This patient during his fever took no medicine but the effervefcing mixture, the doſe of laudanum excepted.—The affuſion was uſed four times.

CASE III.

Dec. 8, 1791. A woman aged fifty-ſeven, who had caught the infection, in her attendance on a poor family labouring under fever, came under my care in the hoſpital, fifty hours after the firſt attack. She was of courſe in the third day of fever; ſhe had the uſual ſymptoms—head-ach, pain in the back and loins, and thirſt: her tongue was furred and her eyes heavy: her pulſe 96, her heat 101°. The affuſion of cold water was performed at noon as uſual. In a few minutes afterwards, the heat under the tongue was 98°, the pulſe 80. Towards evening however, the feveriſh rigor returned, with all the uſual conſequences. As ſoon as the hot ſtage was eſtabliſhed, the affuſion was repeated, and with the uſual happy effects.

Dec. 9.—Noon—pulſe, 90 and feeble—reſpiration eaſy—heat 100°.—The affuſion was immediately employed, and again repeated in the evening—perſpiration, coolneſs, quiet, and ſleep, were the conſequence.

Dec. 10. This day the affuſion was twice repeated as yeſterday, viz. at noon, and at ſix in the evening.—At eight in the evening the pulſe was 76, the ſkin ſoft, the heat 97°, the reſpiration eaſy and natural.—The fever returned no more. This patient uſed no other remedies but an enema and after it an opiate, every night. The affuſion was uſed twice every day, in all ſix times.

CASE IV.

Feb. 2. 1792. S. C. a healthy man, aged forty-four, about ſeventy-two hours after the firſt attack of fever, became my patient. His pulſe 100—his heat 104°.—The other ſymptoms as uſual—but the pain in the head and back particularly ſevere. Two minutes after the affuſion—pulſe 90, heat 101°; ſix minutes after—pulſe 90 and weak—ten minutes after, pulſe 90, heat 100°.—This patient felt great refreſhment, and was entirely relieved of the pain in the head and back.—In the evening however the exacerbation of fever was ſevere, and the head-ach returned with violence.—He paſſed a reſtleſs night. About four o'clock in the enſuing morning the affuſion was repeated by his own deſire. At 9, A. M. a general and gentle perſpiration covered the ſurface of the body; the pulſe was 84, the tongue moiſt, the ſkin cool, and the pains of the head and back entirely gone off. In the afternoon, however, the fever returned though in a ſlighter degree.

The affuſion was repeated the fourth time with the ſame happy effects, and after this he had no return of the diſeaſe.

Thus it appears, that the cold affuſion uſed on the third and fourth days of fever, does not uſually produce an immediate ſolution of the diſeaſe; but that it inſtantly abates it, and by a few repetitions brings it to a happy termination in two or three days.

CASE V.

Oct. 25. M. S. came under my care on the 24th Dec. 1791, on the ſeventh day of typhus, with the uſual ſymptoms--pulſe 108, heat 100°.—The cold affuſion was immediately directed—two minutes afterwards the heat was 96°—three minutes afterwards 98°—the pulſe 98. This patient experienced great relief. The affuſion was repeated on this and the following day—a gentle diaphoreſis always ſucceeded it, with tranquil ſleep, and on the third day the fever was completely removed.

CASE VI.

A young lady of 19, in the 7th or 8th day of typhus, became my patient, Sept. 26, 1794. Her pulſe was 112 and feeble, heat 101°. She had great

pain

pain in the head and much proſtration of ſtrength—her eyes were ſuffuſed and dull—her tongue furred—her ſpirits greatly depreſſed. Saline medicines were uſed for this patient, with lemonade for her uſual drink, and moderate quantities of wine were given mixed with water. The burning ſenſation in the palms of her hands and temples was aſſuaged by frequent ſpunging with vinegar, and every evening at ſix, P. M. three or four gallons of cold brine were thrown over her. The happy effects ſo frequently deſcribed, were in this caſe particularly ſtriking:—The pulſe fell almoſt immediately to 90, the heat to its natural ſtandard, and the head-ach vaniſhed—a gentle diaphoreſis followed, with eaſy ſleep:—in a few hours, however, the feveriſh ſymptoms returned, and towards the hour of ſix in the evening the fever was at its higheſt ſtate of exacerbation. At this hour therefore the affuſion was repeated with the ſame happy effects—though the fever returned as before, it was in a milder form; the ſame practice was continued, and on the ſecond of October, ſhe was entirely free from the diſeaſe.

CASE VII.

F. G. a ſoldier of the 30th regiment, aged 33, fell under my care on the 9th June, 1792, during the prevalence of the epidemic in that regiment, of which

I have already given an account. He was in the 9th day of the diſeaſe—his pulſe 100 and feeble—his heat 104°—his thirſt was very great—his tongue foul and black—frequent cough occurred, with ſtreaks of blood in the expectoration—and petechiæ appeared all over his body. His mind was at all times confuſed, and occaſionally he was completely delirious. I directed that his ſtrength ſhould be ſupported by adminiſtering a bottle of wine every day, with an equal quantity of gruel;—that every night he ſhould take an opiate draught, and that a complete operation of his bowels ſhould be procured by a clyſter adminiſtered daily, and if this did not ſucceed, by a few grains of calomel. I alſo directed that a bucket full of ſalt-water ſhould be thrown over him immediately, and repeated according to circumſtances. In a few minutes after the affuſion, the heat was 98°—the pulſe 98—his mind was more calm and collected: two hours afterwards he had relapſed into nearly his former ſtate, but the night was paſſed more tranquilly. The whole of this practice was continued, with nearly the ſame reſult, till the 12th day of the diſeaſe, the affuſion having always been performed in the evening, and occaſionally at noon. The fever continued its uſual period, but on the 12th day, the heat having ſunk to its natural ſtandard, the cold affuſion was thenceforth omitted; we however, ſpunged the whole body once or twice a day with vinegar.—

vinegar.—The patient was in a ſtate of convaleſcence on the 18th day from the firſt attack.

I have related this caſe the more circumſtancially, becauſe it contains the particulars of my practice in the epidemic in which it occurred, at the ſame time that it affords an example of the effects of the cold affuſion uſed in the more advanced periods of fever. In ſuch inſtances, as might be expected, it does not procure the ſame advantages, as in the earlier ſtages, when the ſtrength is leſs impaired, and the morbid actions leſs firmly aſſociated; nevertheleſs, it is evidently advantageous while the heat of the patient exceeds the natural ſtandard, though it ought to be employed with caution in the more advanced ſtages of the diſeaſe, and according to my later experience, of a temperature from fifteen to twenty degrees only, below the human heat. In the greater part of the caſes that I have related, the water employed was the pump water of our hoſpital, ſaturated with ſea-ſalt, and of a temperature from 40° to 50° of Fah^{t}.

The cold affuſion may alſo be employed with ſucceſs in intermittent fevers, as I have found by repeated trials, and as the following caſe will demonſtrate.

CASE VIII.

Ann Hall, aged 22, was admitted into the Infirmary, July 19, 1792, under an obſtinate quotidian of three months ſtanding—ſhe had from time to time taken the bark, but as the great delicacy of her ſtomach, would not permit her to uſe it in ſufficient quantities, ſhe was become very feeble and much emaciated. A gentle emetic was adminiſtered to her in the firſt inſtance, and on the commencement of the hot ſtage of the paroxyſm, twenty drops of the tincture of opium were directed, after the practice of Dr. Lind. During the intermiſſion, the bark was ordered to be taken in ſuch doſes as her ſtomach would bear; ſhe was put on a nouriſhing diet, and was ordered a pint of port wine every day. This plan was purſued for fourteen days, but without ſucceſs; the paroxyſms returned daily, though with ſome irregularity—her ſtrength was however rather improved. Still the delicacy of her ſtomach continued, and the bark, except in very ſmall doſes, was conſtantly rejected.

On the 8th of Auguſt, two hours before the expected acceſſion of the fever, four gallons of brine were daſhed over her, of the temperature of 66° of Fah^{t}. and this day ſhe eſcaped the attack. In the interval between this and the period of the next return, ſhe took the bark in larger quantity, the tone of her ſtomach being improved ; but on the 10th, two hours later than uſual, the paroxyſm returned with unuſual ſeverity. *After the hot ſtage was completely formed*, the brine was poured over her, as before—the ſymptoms inſtantly abated ; ſhe fell into a gentle perſpiration, with profound ſleep. She afterwards continued the bark as before, and from this time forwards was free of diſeaſe.

It would be eaſy to multiply theſe details, but their uniformity has already perhaps rendered them tedious ; a few general obſervations therefore ſhall conclude this diviſion of the ſubject.

CHAP. VI.

General Obſervations.

1. THOUGH the patients were often ſtartled at the firſt propoſal of daſhing the cold water over them, yet after one trial, there was ſeldom any difficulty in perſuading them to have it repeated. The effects were in general highly grateful and refreſhing to their ſenſations, the extinction or abatement of fever was commonly followed by more or leſs diaphoreſis, and this again by refreſhing ſleep.

2. At firſt I uſed freſh water—afterwards freſh water mixed with vinegar—and laſtly, a ſaturated ſolution of ſea-ſalt in water. In the inſtance of the 30th regiment I uſed the water of the river, which contains about a thirty-third part of ſalt, as has been already mentioned, and this I com-

monly employ in private practice.* I was led to prefer ſalt-water to freſh, on account of the ſtimulating effect of ſea-ſalt on the veſſels of the ſkin, by which I apprehend the debilitating action of cold is prevented. Salt-water either for the purpoſe of immerſion or affuſion is more grateful to the patient than freſh water, and it is well known that it may be applied to the ſurface for a length of time, with much leſs hazard. The countenances of perſons immerſed in ſea-water, and eſpecially in ſaturated brine, for ſome time together, preſerve the luſtre of the eye and the ruddineſs of the cheek longer than thoſe in freſh water, of the ſame temperature, and ſuch perſons exhibit the vital re-action ſtronger when removed from it. I preferred the brine to vinegar, as being cheaper, and more eaſily procured of the neceſſary quantity: otherwiſe, it is well known how grateful vinegar is to patients in fever, and perhaps a mixture of vinegar and water of the proper ſtrength might be preferable even to brine. But though I gave the preference to brine over freſh water, I have very often uſed the laſt, and it is ſeldom that any danger can reſult from the want of a ſaline impregnation, where the cold is employed in ſo ſtimulating a form as that which has been deſcribed, and for ſo temporary a duration.

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* It may be ſuppoſed that the degree of impregnation of the river water with ſalt, muſt depend on the time of the tide when it is taken up. Thus, that it muſt be ſalter at the height of the flood, when the tide has run ſix hours from the ſea, than at the period of low water, when it has run nearly the ſame time from the land.—I expected to find this the caſe, and endeavoured to aſcertain the difference; but in a trial which I made by evaporating 40lbs. of water taken up at the height of the flood tide, and the ſame quantity taken up at low water, I could perceive no difference in the proportion of ſalt, a circumſtance which I am unable to explain. The ſmall difference that there was in the reſiduum in favour of the water taken up at the top of the tide, aroſe evidently from its being mingled with a large portion of dirt—the water at the top of the tide is mixed with dirt and feculencies, that taken at the loweſt point of ebb is nearly pure.

3. In taking the heat of the patient, I have generally uſed a ſmall mercurial thermometer of great ſenſibility, with a moveable ſcale, made for me by Mr. Ramſden, after a form invented by the late Mr. Hunter, and uſed by him in his experiments on the heat of animals, and I have introduced the bulb under the tongue with the lips cloſe, or under the axilla indifferently; having found by repeated experiments, that the heat in theſe two places correſponds exactly, and gives a juſt indication of the

heat of the ſurface of the body where ſheltered by the neceſſary teguments, and ſecluded from the contact of the external air.

Finding, however, conſiderable riſque in uſing the ſtraight tubed thermometer in contagious diſeaſes, I got ſome inſtruments of this kind made with a ſmall bulb curved at the end. The bulb being introduced under the tongue or the axilla, the obſerver can ſtand behind the patient, and mark the riſe of the mercury, without coming into the immediate ſphere of his reſpiration.

CHAP. IX.

Precautions requiſite in uſing the cold affuſion, illuſtrated with Caſes.

1. IT was before remarked, that the cold affuſion cannot be uſed with ſafety during the cold ſtage of the febrile paroxyſm: The following caſe will illuſtrate this truth. In the ſummer of 1792 I was requeſted by Mr. Hoffman, an ingenious Pruſſian gentleman, and a ſurgeon in the army then under the command of the Duke of Brunſwick, to give him an opportunity of ſeeing the method of uſing this remedy. At that time there was a patient labouring under a tertian intermittent under my care in the Infirmary, on whom it could with propriety be exhibited. Accordingly a time for meeting Mr. Hoffman in the fever-ward was appointed, when the hot ſtage of the paroxyſm might be expected to be fairly formed. It happened however that the acceſſion of the fever had occurred an hour later this day than might have been expected, and when we arrived, the patient was ſtill in the cold ſtage of the paroxyſm; the aſſiſtants however proceeded: he was

taken out of his bed ſhivering, his pulſe ſmall and frequent, his extremities ſhrunk and cold. In this ſtate the brine was daſhed over him as uſual, but not with the uſual happy effects:—his breathing was for ſome minutes almoſt ſuſpended; his pulſe at the wriſt was not to be felt; the pulſations of the heart were feeble and fluttering; a deadly coldneſs ſpread over the ſurface; and when reſpiration returned, it was ſhort, irregular, and laborious.—After the uſe of frictions on the ſurface, and particularly on the extremities—of a ſteady warmth applied ſome time to the ſcrobiculus cordis—and of cordials cautiouſly adminiſtered in ſmall quantities—the pulſe at the wriſt returned; but for ſome time it was exceſſively quick and feeble. He recovered however in the courſe of an hour, and it was found that the paroxyſm of fever had been extinguiſhed: but the circumſtances firſt related were evidently full of danger, and they produced at the time much apprehenſion and uneaſineſs. The ſame remedy was however uſed in the hot ſtage of the enſuing paroxyſm, and with the uſual happy effects.—Other caſes to the ſame purpoſe might be adduced if it were neceſſary.

I have frequently uſed the cold affuſion in the hot ſtage of the paroxyſm of intermittents, and almoſt always with the immediate ſolution of the fit; but in general, if no remedy be uſed in the intermiſſion, the fever returns at the uſual period. In ſome inſtances, however, the ſucceeding paroxyſm has been prevented by uſing the cold affuſion about an hour previous to the period of its expected return, and the diſeaſe been ultimately removed by continuing this practice through four or five of the following periods.

The uſe of the cold affuſion in the abſence of fever, requires however a conſtitution in a great meaſure unbroken; and many of the intermittents which we ſee in Liverpool, being tranſmitted to us from the warm climates, adhere to conſtitutions in which this practice is not perfectly ſafe. In ſuch caſes, it may notwithſtanding be adopted in the hot ſtage of fever with ſafety and advantage. Indeed it aught never to be forgotten, that an application of cold which is ſafe in the violence of fever, is not ſafe when the fever is removed. Injury has ſometimes occurred from continuing the cold affuſion in the period of convaleſcence.

2. Neither is the cold affuſion ſafe after the ſweating ſtage of fever has continued ſome time, and the body is paſſing through that cooling proceſs. The following caſe will illuſtrate this poſition. In the ſummer of 1791, a boy of eight years of age, in whom I am peculiarly intereſted, was attacked by fever. On the third day his pulſe roſe to 130,

and 140 in the minute, and his heat to 106° and 107° of Fahᵗ. His thirſt was very great, and delirium commenced on the ſecond day, and continued without intermiſſion. Various methods had been employed to abate the fever, and particularly to excite perſpiration, but unſucceſsfully. His heat was not leſſened by repeated ſpunging of the ſurface of the body with cold vinegar and water; and after a copious bleeding, all the ſymptoms were as alarming as before. It ſeemed hazardous to repeat this evacuation, as the blood exhibited no ſize, and there was a ſuſpicion that the diſeaſe originated in contagion. The patient had taken antimonials without any apparent effect, and after watching the ſtate of the thermometer with the bulb at the axilla, upwards of an hour, though the mercury had ſunk a ſingle degree in that interval, it ſtood at the end of the time as high as 107°. In this ſtate of things we reſolved on trying the cold affuſion, and every thing being prepared, he was ſtripped naked and lifted out of bed. As we were about to throw the water upon him, it was obſerved that a perſpiration had broken out all over him, but the heat being ſo great, we perſiſted in our purpoſe, and four gallons of freſh water of the temperature of 60° were daſhed upon him; the effects were altogether ſurpriſing. On replacing him in bed, the mercury in the thermometer (the bulb at the axilla as before) roſe to 98° only, and the burning heat of the extremities was converted into a coolneſs that was rather alarming; the pulſe had ſunk in frequency to 90° but was full and ſteady. Gentle frictions were applied to the legs and feet, but they were not long continued, for the general warmth ſpeedily returned; the heat in the trunk of the body roſe in about an hour to 100° and the pulſe to 100. His dilirium went entirely off; the fur on his tongue ſpeedily diſappeared; and twenty hours afterwards he was found free of every complaint but debility.

Subſequent experience has however convinced me that though in this caſe the termination was ſo happy, the cold affuſion was not unattended with hazard. Perſpiration had commenced and the heat was ſinking. It had perhaps ſunk more at the moment when the affuſion was performed, than was indicated by the thermometer, for the bed-clothes often keep the body from cooling under perſpiration to the degree that would otherwiſe be produced. In this caſe when the ſurface is ſuddenly expoſed to the external air the heat ſinks rapidly. If the perſpiration had continued an hour longer before this remedy was uſed, the heat would have been ſtill more diminiſhed; a torpor of the veſſels of the ſurface, and of the extremities, would have been produced, followed by a great, and probably a dangerous re-action of the centre. This obſervation will be illuſtrated in the ſequel.

In recommending the affuſion of cold water as a remedy in fever, an expreſs exception is therefore made againſt its uſe during the feveriſh chill, or after the perſpiration has begun to flow profuſely, and more eſpecially after it has continued to flow profuſely for ſome time. An exception is alſo made againſt its being employed in the latter end of fever, when the ſtrength is much exhauſted, and the heat is ſometimes as low or lower than the temperature of health. While, however, the heat riſes one or two degrees above the healthy ſtandard, this remedy may he uſed even in the latter ſtages of fever. I have employed it with advantage on the 11th, 12th, and 13th days. In inſtances of this kind it will however be prudent to make the degree of cold very moderate, as has been already obſerved; and as it is ſcarcely to be expected that at an advanced period of the diſeaſe the progreſs of it can be ſtopped, or its duration much leſſened, it may perhaps anſwer every purpoſe to employ in ſuch caſes the *tepid* ablution. I have indeed often contented myſelf with ſpunging the body all over with tepid vinegar, or vinegar and water, from the 9th or 10th day forwards; but I have frequently in caſes where the heat continued high, directed the general affuſion of tepid water, by which the heat may always be ſpeedily and effectually reduced, *when that is the only object in view.*

Under theſe reſtrictions, the affuſion of cold water may be uſed with perfect ſafety in the low contagious fever of this country, and the facts already ſtated will ſhow that it is a remedy of great power and efficacy. In the firſt ſtages of fever, it appears very generally to cut ſhort the diſeaſe almoſt inſtantaneouſly; and even when it fails of this effect, as is uſually the caſe when it is applied in the more advanced ſtages, it nevertheleſs moderates the violence of the ſymptoms, and ſhortens the duration of the fever.

CHAP. VIII.

General remarks on fever. Hiſtory of a caſe of fever in which the affuſion of cold water was not ſalutary.

SINCE the introduction of ſcientific arrangements into medicine, diſeaſes have been much reduced in number, and their nature has been more clearly underſtood. This is eſpecially true of continued fever, which is exhibited by Dr. Cullen under three genera only, Synocha, Typhus, and Synochus. Of theſe genera, however, the Synocha, or pure inflammatory fever without topical inflammation, is confeſſedly a very rare occurrence in this iſland; the venerable profeſſor uſed to declare that he had not met with a ſingle inſtance of it in forty years practice.—And the typhus and ſynochus ſeem to be conſidered by him as the ſame diſeaſe, modified differently, by the difference of climate, ſeaſon, and conſtitution. Both are deſcribed as contagious, and as occaſionally producing each other. Doubtleſs the typhus, or low contagious fever, is the prevailing fever of this iſland, and of Europe. It is the epidemic of all our great towns, of our jails, hoſpitals, and manufactories; its origin and progreſs are clearly aſcertained, and its ſymptoms generally underſtood. It is to this fever that the preceding obſervations chiefly apply.

I have my doubts, however, after much reflection and obſervation, whether we have not ſimplified too faſt in our noſological arrangements of fever. The dreadful diſeaſe which prevailed lately at Philadelphia, and which now ravages the Weſt Indies, cannot perhaps be included without ſome violence within our ſyſtems of Noſology; and its fatality under all the eſtabliſhed modes of treatment, whilſt it excites our deepeſt regret, muſt ſerve to abate the pride of modern ſcience.—Even in our own iſland, it appears to me that caſes of fever occaſionally occur, which cannot be referred with advantage to any of the genera of Dr. Cullen*. The following is a deſcription of a fever of this kind; I have not met with it often, but when I have met with it, it has very generally proved fatal, under the eſtabliſhed modes of treatment; and I am ſorry to ſay, that in the only inſtance of this fever in which I have tried it, the affuſion of cold water proved unſucceſsful alſo.

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* I am aware that all queſtions reſpecting Noſological arrangement have a tendency to degenerate into verbal diſputes, and I willingly avoid them, referring for my accuracy to thoſe who have ſtudied diſeaſes, not in books only, but in the volume of nature.

The fever in queſtion does not ſeem to originate in contagion, or to propagate itſelf by contagion. I have never been able in a ſingle inſtance to trace it to that ſource, nor have I ever found it to be communicated from the patient to any of his attendants. The caſes which I have ſeen have occurred chiefly in the winter ſeaſon, in perſons in the flower or vigour of life, poſſeſſed of conſiderable ſenſibility of mind, and in the habits of more than ordinary mental exertion. After ſome days of indiſtinct catarrhal complaints, the fever comes on (in general after ſome accidental expoſure to cold) with a very violent and long continued attack of chills and rigor, and to this, as is uſual, ſuceeeds a ſtate of heat and re-action. The patient complains of intenſe head-ach and of oppreſſion at the præcordia, with occaſional but not ſevere cough, and with ſome increaſe in the frequency of reſpiration. His pulſe is not remarkable as to frequency or ſtrength; his ſleep is not particularly diſturbed; and for ſome days the complaint goes on as if produced by catarrhal fever. From the firſt, however, there appears a great quickneſs and impatience about the patient: he talks more rapidly than uſual; apprehends you quickly, and anſwers you inſtantly. He cannot, however, command his attention long, and is fatigued with the effort. His heat, which was at firſt moderate, becomes very great on the 7th and 8th day, reaching 109° or 110° of Faht.; he becomes delirious and talks inceſſantly. Throughout the fever, his ſenſes of hearing and taſte are uniformly acute, and this is true alſo of his ſenſe of feeling. Great as his heat is, he is much alive to the impreſſions of cold on the ſurface of the body, and ſhrinks from them. At times he appears ſurpriſingly calm and natural, gets out of bed and dreſſes himſelf, inſiſting that he is well. Often he ſtarts up ſuddenly in bed and opens his curtains, ſeeming to look round the room for ſome perſon he ſuppoſes preſent; and ſometimes he rings the bell violently, if within his reach, without apparent object. Indiſtinct conceptions riſe and vaniſh in his mind, and the impreſſions of ſenſe are confounded with the ideas of imagination. As the fever advances, the reſpiration becomes more hurried and laborious, and the pulſe more frequent and feeble; and towards the latter end of the diſeaſe, but not before, perſpirations break out, at firſt partial, and at length general and profuſe, which however, though they reduce the heat, do not otherwiſe relieve him.

The pulſe ſinks; the body is covered with petechiæ; wine, bark, opium, and bliſters, afford no relief: the patient dies on the twelfth or thirteenth day of fever, and after death the body runs rapidly into putrifaction.

I have ſeen this fever treated by veneſection and antimonials in the early ſtages, with a ſtrict attention to the antiphilogiſtic regimen; and by bark and cordials, as the ſtrength began to decline; but without ſucceſs. I have alſo ſeen it treated from the firſt on the ſame plan as typhus, but with an equally unfortunate iſſue. In a caſe of this fever which occurred lately, I made uſe of the cold affuſion, and as the mercury roſe in the thermometer with the bulb under the tongue to 108°, I employed this remedy with ſome degree of confidence. The effects did not correſpond with my former experience, or with my hopes. The patient felt the cold moſt acutely, but was not relieved. His pulſe did not diminiſh in frequency; his heat ſubſided very little, and that for a few minutes only; neither diaphoreſis nor ſleep followed. This remedy was not repeated, but the ſurface of the body was ſpunged from time to time with vinegar, without however producing ſenſible benefit or refreſhment.

I have already mentioned that the affuſion of cold water is not to be uſed after a profuſe perſpiration has taken place; and that it is not to be uſed in the cold ſtage which begins the paroxyſm of fever, nor till the hot ſtage be fairly formed. In the typhus, however, this laſt reſtriction ſeldom requires us to wait long, the affuſion may be uſed in general in twenty-four hours from the original attack, and often much ſooner. The caſe I have juſt related is the only inſtance, out of many hundred trials, in which I found, that even on the ſixth day of fever, with the actual heat of the body far above the temperature of health, the affuſion of cold water was neither ſalutary nor refreſhing. I have however to obſerve, that notwithſtanding the great heat of the body, producing the utmoſt reſtleſſneſs and anxiety, the *ſenſation of heat* was interrupted by chillneſs, on the ſlighteſt application of cold, and that the ſurface and extremities not only felt chilly, but grew cold even on the acceſſion of the external air. In reality, through the greater part of the fever, the ſtate of the patient had a conſiderable reſemblance to what we ſee in the paroxyſm of an intermittent, when the cold ſtage is terminating, but the hot ſtage not fairly formed—when the heat as well as the blood, is accumulated in the centre of the ſyſtem, and the vital power is ſtruggling to give them that propulſion to the ſuperficies which terminates in profuſe perſpiration, and carries off the diſeaſe. I have little doubt that immerſion in the tepid bath continued for ſome time (of the temperature that feels comfortably warm to the ſkin) is the proper remedy in the fever I have deſcribed, as it doubtleſs is in the ſtruggle of the paroxyſm of intermittent; and when an opportunity offers, I mean to aſcertain the truth on this point. But this opinion will be illuſtrated when we come to ſpeak of the warm bath more particularly.

If any one ſhould contend that the fever I have deſcribed is in reality only a variety of the typhus, or ſynochus, I ſhall not be diſpoſed to conteſt the matter. The queſtion concerning *identity*, leads to endleſs diſputes in every branch of ſcience where it occurs, and he muſt know little of noſology, who ſuppoſes *it* has yet received a conſiſtency, that would render ſuch a diſcuſſion profitable. It is ſufficient for me to obſerve, that the ſymptoms of the two diſeaſes are in a conſiderable degree different, though with that general ſimilarity that belongs to all caſes of fever; that the ſtate of the nervous ſyſtem as to impreſſibility is widely different; and what is of moſt importance, that the methods of treatment, which according to my experience almoſt invariably ſucceed in the one diſeaſe, are unſucceſsful in the other. Every practitioner knows, that in typhus, the ſenſe of hearing is generally obtuſe; and the ſame may be ſaid of the taſte, ſmell, and touch; whether the obſervation is applicable to the ſight alſo, and under what reſtriction, appears to me doubtful. The acuteneſs of all theſe ſenſes in the fever which I have deſcribed, is very remarkable, and particularly in regard to the ſenſibility of the ſurface. I have obſerved this ſymptom to be produced by ſeveral narcotics and by ſome poiſons. It is very remarkable in the hydrophobia; and in the laſt days of a perſon who died of hunger, the ſenſes of touch and viſion were extremely acute.*

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* This caſe will be found in a ſubſequent part of this volume.

CHAP. IX.

Of the uſe of the affuſion of cold and tepid water in ſmall-pox, with caſes.

THE ſingular degree of ſucceſs that on the whole attended the affuſion of cold water in typhus, encouraged a trial of this remedy in ſome other febrile diſeaſes. Of theſe the ſmall-pox ſeemed more particularly to invite its uſe. The great advantage that is experienced in this diſeaſe by the admiſſion of cool air, ſeemed to point out the external uſe of cold water, which being a more powerful application, might be more particularly adapted to the more malignant forms of ſmall-pox. The reſult correſponded entirely with my expectations. Of a number of caſes in which I witneſſed the happy effects of the affuſion of cold water in ſmall-pox, I ſhall give the following only.

CASE I.

In the autumn of 1794, J. J. an American gentleman in the 24th year of his age, and immediately on his landing in Liverpool, was inoculated under my care; the prevalence of the ſmall-pox rendering it imprudent to wait till the uſual preparations could be gone through, or indeed till the fatigues of the voyage could be recovered. He ſickened on the ſeventh day and the eruptive fever was very conſiderable. He had a rapid and feeble pulſe, a fœtid breath, with pain in the head, back, and loins. His heat roſe in a few hours to 107°, and his pulſe beat 119 times in the minute. I encouraged him to drink largely of cold water and lemonade, and threw three gallons of cold brine over him. He was in a high degree refreſhed by it. The eruptive fever abated in every reſpect—an incipient dilirium ſubſided, the pulſe became ſlower, the heat was reduced, and tranquil ſleep followed. In the courſe of twenty-four hours the affuſion was repeated three or four different times at his own deſire; a general direction having been given him to call for it as often as the ſymptoms of fever returned. The eruption, though more numerous than is uſual from inoculation, was of a favourable kind. There was little or no ſecondary fever, and he recovered rapidly.

In ſituations where the eruptive fever of ſmall-pox is clearly diſtinguiſhable, and where it does not abate ſufficiently on the admiſſion of cold air, the affuſion of cold water may be reſorted to with confidence and ſafety, regulated however in this application, as in every other, by *the actual ſtate of the patient's heat, and of his ſenſation of heat.* In the confluent ſmall-pox, however, after the eruption is completely formed, this remedy cannot perhaps be uſed with advantage. The following caſe will illuſtrate this poſition.

CASE II.

H. A. aged 23, an American mariner fell under my care (*Dec.* 7,) on the third day of the eruption of the ſmall-pox; that is, on the ſixth day of the diſeaſe. His pulſe 114 and feeble, his heat 100°. His head, back, and loins ached ſeverely—thirſt great—ſkin-livid—ſmall-pox confluent.

He was put on a milk diet—gentle mercurial purgatives were ordered from time to time, and an opiate every night at bed-time. Lemonade was given largely, at firſt by itſelf, and afterwards mixed with wine, and the affuſion of cold water was directed in the uſual way. In ten minutes after the affuſion, the pulſe was 96, the heat 98°; the livor of the ſkin was much diminiſhed, but the pains were not relieved.

Dec. 8. Noon—pulſe 96, ſoft and regular—thirſt gone—reſpiration ſlow and natural—heat 97°.—The affuſion was ordered to be repeated; ten minutes after pulſe 84 and feeble—heat 84°.

Dec. 9. Noon—pulſe 88, heat 93—the cold affuſion was not repeated in this very reduced ſtate of heat; the decoction of bark was ordered, and a pint of wine daily in lemonade.

Dec. 10. Noon—pulſe 116, and full—heat 98°, reſpiration ſtill eaſy—expectoration conſiderable and viſcid—thirſt leſs—eyes quite cloſed—head ſwelled—a complete union of the puſtules on the face. Bark and wine continued, with the opiate at night.

Dec. 12. Pulſe 118—heat 96°. A bucket full of water of the temperature of 92° was poured over him. He appeared refreſhed at the moment; ten minutes after pulſe 112, heat 94°. Complained of being chilly. Reſpiration ſtill eaſy—free of pains and his face fallen. Complained of his throat. A bliſter was applied to it all round.

Dec. 13. Noon—pulſe 118—heat 96°—reſpiration ſtill free, but his throat very ſore. Medicines were continued, but the affuſion of tepid water was not repeated.

Dec. 14. Noon—pulſe 138—heat 100°—reſpiration is now become laborious, and the expuition difficult. The throat is much ſwelled. He was frequently ſpunged with tepid water, and the medicines continued.

Dec. 15. Noon—unfavourable ſymptoms increaſed.

Dec. 16. Noon—vomiting came on which was relieved by opium. His ſenſes and his intellect remained acute to within an hour of his death, which happened at eight o'clock in the evening of this day.

If this caſe be more detailed than ſeems neceſſary, let this be excuſed, as it is the firſt on record, in which the actual heat in confluent ſmall-pox has been recorded. It is here given accurately from the period when the diſeaſe came under my care.

In regard to the effects of the cold affuſion, it may be obſerved, that this remedy was not uſed during the eruptive fever, nor till three days after the eruption had appeared, and the character of the diſeaſe was decided. In the ſtage in which it was employed, the fever and the heat were abating, as is uſual after the eruption; and in all caſes in which the heat is ſinking, the application of cold muſt be made with great caution, as has already been mentioned. After the ſecond affuſion (on the 8th) the heat ſunk below its natural ſtandard, and continued below it for ſome time; ſo that this remedy became inadmiſſable. The diſeaſe went through its uſual courſe. The tepid affuſion on the eighth day of the eruption *(Dec.* 12) was uſed in part to waſh off variolous matter, and in part to produce refreſhment. The heat which was before 96°, ſunk two degrees, ſo that it could not with ſafety be continued, for experience has proved that the tepid affuſion is a powerful means of diminiſhing heat. The heat roſe again with the ſecondary fever, and the patient died of the affection of the throat, as I believe is general in the confluent ſmall-pox.* It will be at once perceived, on the principles already laid down, that in a diſeaſe like this, the affuſion of cold water could only be eſſentially uſeful during the eruptive fever. It is during the eruptive fever that the quantity of the aſſimilation is determined, as well as its kind. This is invariably found to bear an exact proportion to the eruptive fever, and whether we conſider the eruptive fever as the cauſe or effect of the aſſimilation, there is every reaſon to expect, from the laws of the living ſyſtem, that the diminution of this fever will diminiſh the quantity, and meliorate the quality of the variolous eruption.

In the caſe juſt related, the heat during the eruptive fever (from trials in ſimilar ſituations) had doubtleſs riſen to 106° or 108°; but it had ſunk to 100°, before the cold affuſion was employed. It may eaſily be conceived that this remedy could have been employed to a much greater extent, and that its effects would have been far more ſalutary, if it had been uſed throughout the previous fever. That it would have eſſentially altered the character of the diſeaſe, I preſume not to aſſert. This however, I can declare, that in all the caſes in which I have uſed the affuſion of cold water during the eruptive fever, however ſevere the ſymptoms may have been, theſe ſymptoms inſtantly abated, and the diſeaſe aſſumed a benignant form. The caſe of Mr. Johnſton (Caſe I.) already given, will illuſtrate this obſervation; and ſix or ſeven others I might adduce to the ſame purpoſe. As yet my experience extends no farther.

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* See Zoonomia, Vol. II. page 237. The article on the confluent ſmall-pox is very valuable, and diſplays the uſual acuteneſs and comprehenſion of its author.

The inoculation of infants is ſo very ſeldom followed by any ſerious diſeaſe, that as far as reſpects them, the affuſion of cold water may be ſeldom required. The inoculation of adults is not, however, quite ſo ſafe. Inoculation is ſeldom indeed performed in our iſland on adults that are natives, but foreigners frequently require it; and in Liverpool, our intercourſe with America renders it often neceſſary to perform it on adults from that continent. We may alſo obſerve, that when the natural ſmall-pox is epidemic, the eruptive fever will be generally diſtinguiſhed, and wherever it is diſtinguiſhed with ſymptoms of violence, inſtead of truſting to cool air only, the cold affuſion, or cold bath, are ſtrongly recommended. To our brethren acroſs the Atlantic this is more eſpecially addreſſed. In America, as well as many parts of the old continent, in conſequence of the neglect of early inoculation, the natural ſmall-pox at times ſpreads alarm and devaſtation throughout extenſive diſtricts. In this iſland the ravages of the natural ſmall-pox are on the whole very great, but they occaſion little diſturbance or alarm. The practice of inoculation among the more opulent claſſes of ſociety, keeps up the contagion in all our populous diſtricts, and deſtroys all oppoſition to the caſual progreſs of this contagion, in the only quarter in which oppoſition can be effectual. Though therefore it is demonſtrable, and has indeed been demonſtrated, that the deſtruction of the natural, or rather the caſual ſmall-pox, might be entirely avoided, yet, as it falls almoſt wholly on the families of the poor, and as it has been an evil that has been long, and that is familiarly known, we ſubmit to it through habit, as if it were inevitable—one of theſe natural ills, *that fleſh is heir to.* The alarm produced by the caſual contagion is therefore ſeldom ſo great in our large towns, as to give a practitioner frequent opportunities of treating the eruptive fever of the confluent ſmall-pox, the only ſtage of that diſeaſe in which medical treatment is likely to be of much avail. Where ſuch an opportunity does occur to the enlightened reader of theſe pages, it is hoped that the affuſion of cold water, or the cold bath, will not be neglected*. The Chineſe, it is ſaid, have long followed this practice with extraordinary ſucceſs.

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* The obſervations on the poſſible prevention of the caſual ſmall-pox will doubtleſs ſuggeſt to the medical reader, the " Sketch of a Plan" for that purpoſe publiſhed in 1793, by my reſpectable neighbour, Dr. Haygarth. That this plan is in itſelf practicable, and that it would be effectual, I have little doubt. Unfortunately, it requires the aſſiſtance of government (as I remarked at the time) and this I fear is a powerful objection. It is poſſible however that ſome ſcheme of this kind (including I hope all contagious diſeaſes) may one day or another be attached to ſome comprehenſive plan for the management of the poor.

The theory that ſuggeſted Dr. Haygarth's plan, but which formed no eſſential part of it, involved him in a diſcuſſion on the length of time that variolous matter may be expoſed to the atmoſphere, and retain its infectious quality. On this occaſion, as there was ſome difference of opinion between us, he ſuggeſted ſeveral experiments to me which would doubtleſs have decided the queſtion. (*See pages* 459, 460 *of* " *A Sketch*, &c.") It was fully my intention to have undertaken ſome experiments ſuch as he ſuggeſted, and I even commenced them, but as my attention about this time, (*April* 1793) was

The uſe of the cold affuſion in fever was common among the phyſicians of the infirmary, (Dr. Brandreth, Dr. Gerard, and myſelf) while fevers were received

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was forcibly drawn to other ſubjects, theſe experiments, which required extreme accuracy, were not completed, and my engagements have never ſince permitted me to recommence them. I have, however, ſince that time, inoculated with matter at different periods from its being taken from the patient, and the reſult is as follows. The length of time which variolous matter expoſed to the air retains its contagious quality depends on its ſuperficies. If it be ſpread very thin on a piece of flat or convex glaſs, it loſes this quality much ſooner than when it is collected in a maſs. Spread thin upon glaſs, it ſometimes diſappoints the inoculator at the end of twenty days, though not generally; and I have known it ſucceed in communicating the diſeaſe, even when diffuſed over a large ſurface, at the end of ſeventy-three days. (*See Dr. Haygarth's Sketch page* 447.) But I find from the experience of Mr. Park, as well from my own, that this is not common The inſtance in which I found by experience variolous matter to retain its infectious quality longeſt was the following. On the ſecond of Feb. 1792, I took a conſiderable quantity of this matter on a piece of window glaſs, keeping it as much together as its fluidity would admit. It was expoſed immediately to a ſtream of air, and the ſurface was ſpeedily dry. On the ſecond of March following, after moiſtening a portion of it with a little water, I inoculated three patients, and all with ſucceſs. In the courſe of the ſummer I inoculated with another portion of it, previouſly liquified by the addition of warm water, and with ſucceſs as before. On the twentieth of July, 1793, I again uſed a portion of the ſame matter in the ſame way, and again with ſucceſs; but received into that hoſpital, and its extraordinary ſucceſs there, has rendered it in Liverpool familiar in private practice, and extended it, as I have already mentioned, to ſeveral other febrile diſorders. The ſcarlatina anginoſa has for many years prevailed in Liverpool, and though in general mild, has at particular ſeaſons been malignant and fatal. To this diſeaſe, the affuſion of cold water has lately been applied by my friend Dr. Gerard, and the particulars that follow are mentioned on his authority.

In the latter end of December 1796, all the children of a family in his neighbourhood, five in number, had been attacked in ſucceſſion with ſcarlet fever, four of theſe were recovering, but one was dangerouſly ill, when the father of the family, with whom one of the children had ſlept, was himſelf ſeized with all the ſymptoms of the diſeaſe. He had exceſſive pain in his head and back, and flying pains all over him. He had frequent rigors, loſs of appetite, and ſickneſs, with ſome fluſhing of the face, but without any

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but this ſucceſs was long doubtful, and it was not till the twenty-ſecond day after the operation, that the patient ſickened.—I uſed the ſame matter in June 1794, when it entirely failed me. It is now by me and is not mouldy, nor any ways changed in its appearance. Variolous matter kept ſome time is certainly ſlower in producing the diſeaſe, even where it does ſucceed in the end.

any effloreſcence on the ſkin or affection of the the throat. This was his ſituation when Dr. Gerard was called in, about ſixteen hours after the firſt attack. An emetic, and afterwards a cathartic were ordered, but their operation was ſlow and imperfect, and on viſiting him, ten or twelve hours afterwards, he was not materially relieved.

Entertaining no ſort of doubt of the nature of the attack, theſe ſymptoms foreboded that the epidemic would, in this inſtance, be ſevere; and reflecting on the importance of the patient's life, Dr. Gerard determined to try the affuſion of cold water, from which in typhus he had ſeen ſuch happy effects. Accordingly the operation was performed, and with a reſult that far exceeded his hopes. As he was much debilitated, half a pint of hot wine was given him after it, and on being put to bed the ſymptoms of fever were found nearly gone; a genial warmth diffuſed itſelf over the extremities of his body, followed by perſpiration and ſleep. Next day he complained of a ſlight degree of head-ach and laſſitude; Dr. Gerard therefore ordered the affuſion to be repeated, as well as the warm wine after it; the ſymptoms of the diſeaſe vaniſhed and never re-appeared.

A day or two afterwards a maid who had been hired as an aſſiſtant to attend the ſick children, and who had been about a week in the houſe, was attacked by the preciſe ſymptoms already related, and which had uniformly uſhered in the epidemic. She took an emetic on the firſt attack with little benefit, and ſoon after the cold water was poured over her, the wine being adminiſtered after it. In this caſe the remedies were uſed earlier than in the former one;—they were uſed once only: the febrile paroxyſm was diſſolved, and never returned.

The reſult of theſe caſes communicated by Dr. Gerard, leads to a variety of important reflections. That the affuſion of cold water extinguiſhes the incipient ſcarlatina as well as the typhus, can ſcarcely be doubted; and thus this powerful and ſimple remedy is extended to another, and a moſt important claſs of diſeaſes. That the diſeaſe was extinguiſhed without the ſpecific effloreſcence of the ſkin, or affection of the throat, is a circumſtance not a little curious. It ſeems to demonſtrate that this efflореſcent matter is the product of the eruptive fever; and that the fever itſelf being deſtroyed in the firſt inſtance, the efflореſcent matter is never produced. Thus we are freed from the apprehenſions which a falſe theory might ſuggeſt againſt extinguiſhing a proceſs by which nature was extricating itſelf from an acrimony which the ſyſtem had imbibed.—Thus alſo our concluſion is ſupported that the eruptive fever of ſmall-pox is the cauſe, and not, as ſome have ſuppoſed, the conſequence of the progreſs of aſſimilation, and that the diminution of this fever by cool air, and ſtill more by the affuſion of cold water, actually diminiſhes the quantity of matter aſſimilated, and in certain caſes might perhaps wholly prevent the aſſimilation.* This laſt concluſion is indeed doubtful, as well as all analogical inductions muſt be between different diſeaſes; and particularly where the difference is of this important kind, that the conſtitution in one caſe is ſuſceptible of the diſeaſe once only, while in the other it may probably receive it indefinitely. We may alſo obſerve that the prevention of the aſſimilation of ſmall-pox, by wholly extinguiſhing the eruptive fever, if it were in our power, would not be adviſeable, ſince it muſt leave the patient expoſed to the future influence of that contagion.

I have not had an opportunity of repeating Dr. Gerard's practice in the incipient ſtage of ſcarlatina, but after the efflorеſcence on the ſurface decides the nature of the attack, I have for the laſt fifteen months uniformly preſcribed immerſion in the tepid bath, (from 92° to 96°) and with ſtriking benefit.

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* Dr. Cullen has aſſumed, that in all the various degrees of ſmall-pox, the quantity of matter aſſimilated bears an exact proportion to the bulk of the body, and that the difference in the nature and quantity of the eruption depends wholly on the permeability of the ſkin. This doctrine, which in his lectures he extended to all the other exanthemata, is, in my judgment, one of the weakeſt parts in his moſt valuable work.

Whether the affuſion of cold water is applicable to the other exanthemata, muſt be left to future experience.

In the caſes that I have related to illuſtrate the effects of the cold affuſion, the temperature of the water may be judged of from the ſeaſon of the year. In general it was from 40° to 50° of Fah^t^. In the epidemic which prevailed in the 30th regiment, the water of the river was employed, as has already been mentioned, which as the ſeaſon was uncommonly cold, did not, though in the month of June, exceed 58° or 60°. I have, however, very often uſed the river water in private practice during the ſummer months of the laſt four years, when in general it has been from 65° to 70°, and the effects differed in no reſpect from thoſe already deſcribed. The ſolution of fever, or to ſpeak in language more preciſe, but as yet leſs intelligible, of the morbid febrile catenation, depends on the ſudden, general, and powerful impreſſion on the ſenſations, and this impreſſion is leſs affected by the difference in the temperature from 40°. to 70°. as far as my obſervation extends, than might on a firſt conſideration be imagined. Within theſe limits the efficacy of this remedy, as well perhaps as its ſafety, depends on the ſuddenneſs and momentarineſs of its application. The powerful impreſſion on the ſenſations is much weakened when the water is poured ſlowly on the body, and as the reſpiration is ſuſpended or convulſed during this application, as well as during the act of immerſion in the cold bath, it might in ſome caſes incur hazard to protract it.

CHAP. X.

Of the affuſion of tepid water on the ſurface of the body, in feveriſh diſorders, and of ſpunging the body with water or vinegar. The affuſion of tepid water practiſed in antiquity.

I EMPLOY the term *tepid* when water is heated to that degree which is warm, but not hot to the ſenſations, and which in the way of affuſion is from 87° to 97° of the ſcale of Fah^t^. According to my experience this term, when the body is immerſed, may be applied to water ſome degrees colder, the reaſon of which will be eaſily underſtood by thoſe who reflect, that under immerſion no evaporation from the ſurface of the body takes place. At firſt I imagined that the tepid affuſion might be employed in caſes where the heat of the body is below the degree neceſſary to render the cold affuſion ſafe. I employed it therefore in thoſe ſtages of fever where the heat did not exceed the temperature of health.

A little experience however convinced me that this practice was not without hazard, for I found, that in many caſes, at leaſt, the heat of the living body is lowered as ſpeedily by the affuſion of tepid water, as by the affuſion of water that is cold ;—if I miſtake not, in ſome caſes the heat is lowered more ſpeedily by the tepid water. To thoſe who reaſon reſpecting the heating and cooling of the living body in the ſame manner as reſpecting inanimate matter, this obſervation will appear paradoxical ; I aſſert it however from actual obſervation, and a little reflection will explain the phenomenon. The evaporation from the ſurface is more copious from the tepid affuſion, and on this the cooling of the body very much depends. But this is not all ; the tepid affuſion is little if at all ſtimulating, and does not, like the cold affuſion, rouſe the ſyſtem to thoſe actions by which heat is evolved, and the effects of external cold are reſiſted. Where the object is to diminiſh heat, that may be obtained with great certainty by the repeated uſe of the tepid affuſion, ſuffering the ſurface of the body to be expoſed in the interval to the external air—and if the beams of the ſun are excluded, and a ſtream of wind blows over it, the heat may thus be reduced where cold water cannot be procured ; even in the warmeſt regions of the earth—on the plains of Bengal, or the ſands of Arabia. I have accordingly employed the tepid affuſion very generally in thoſe feveriſh affections where the morbid actions are weakly catenated, depending rather on the ſtimulus of preternatural heat, than on contagion, miaſmata, the morbid contents of the ſtomach and bowels, or local inflammatory affection. Of this kind are a great part of the feveriſh affections of children, in which the tepid affuſion is a valuable remedy. It very generally produces a conſiderable dimunition of heat, a diminiſhed frequency of the pulſe and reſpiration, and a tendency to repoſe and ſleep. I have uſed it alſo in feveriſh diſorders of various kinds where the lungs are oppreſſed, and the reſpiration laborious, and where of courſe the oppreſſion might be dangerouſly augmented, by the ſudden ſtimulus of the cold affuſion. It is alſo applicable to every caſe of fever in which the cold affuſion is recommended, and thoſe may receive much benefit from it, whoſe fears or whoſe feebleneſs deter them from that energetic remedy. I have not however found its effects ſo permanent as thoſe of the cold affuſion, and I have never ſeen it followed by the total ceſſation of regular fever as often occurs after the cold affuſion. In the hectic fever, however, where the actions are leſs ſtrongly catenated than in ſynochus or typhus, the paroxyſm is ſometimes completely extinguiſhed by the affuſion of tepid water* in the commencement of the hot ſtage. In the hectic paroxyſm, the heat ſeldom riſes more than two degrees above the temperature of health in the trunk of the body, and three or four degrees on the extremities. By moiſtening the palms of the hands and the ſoles of the feet with vinegar, its effects may be moderated, for it is from the ſenſation of heat in the extremities that the ſtimulus to the ſyſtem is chiefly derived; and this practice ought not to be neglected, if the tepid affuſion is not employed generally. In all caſes of fever indeed, where the burning heat of the palms of the hands and ſoles of the feet is preſent, this method of cooling them ſhould be reſorted to; it is uniformly ſafe and refreſhing. I have not employed the cold affuſion in the hectic paroxyſm. This diſeaſe generally adheres to a debile ſyſtem; the body parts with its heat in it eaſily; and the lungs being always affected in the pulmonary hectic, the ſudden application of cold to the ſurface might produce unpleaſant and perhaps dangerous effects on the reſpiration. Neither have I tried it in peripneumony or meazles.

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* See this fact mentioned in the Zoonomia, vol. ii. p. 296. where a relation is inſerted by Dr. Darwin, of the author's (Dr. C's.) own caſe of hereditary conſumption.

When the affuſion of water cold or tepid is not employed in fever, benefit may be derived, as has already been mentioned, though in an inferior degree, by ſpunging or wetting the body with cold or warm vinegar or water. The application is however to be regulated like the others by the actual ſtate of the patient's heat and of his ſenſations.

According to my experience, it is not only leſs effectual, but in many caſes leſs ſafe; for the ſyſtem will often bear a ſudden, a general, and a ſtimulating application of cold, when it ſhrinks from its ſlow and ſucceſſive application.

I have alſo uſed the affuſion of *cool* water as a remedy in febrile diſeaſes, but more frequently in paralyſis, and in other diſeaſes of debility. By the term cool, I deſignate the temperature from 87° to 75°. It operates as a gentle ſtimulant, and may be uſed as a milder form of the cold affuſion. Like the cold affuſion its application ſhould be ſudden and momentary, when the object is to increaſe the tone of the ſyſtem or to diſſolve a morbid catenation; where it is employed to moderate inordinate heat, it may be uſed more ſlowly, provided it does not interrupt the catenation on which reſpiration depends.*,

The practice of giving cold water as a drink in fevers was common among the ancients, but the affuſion of it on the ſurface of the body ſeems to have been wholly unknown. This, as far as I can trace it, was firſt practiſed at Breſlaw in Sileſia, as appears from a diſſertation under the title of *Epidemia verna quae Wratiſlaviam, anno* 1737, *afflixit*, to be found in the Appendix to the *Acta Nat. Curios. vol.* x. a work which I have not had an opportunity of conſulting.* While, however, the laws by which the affuſion of cold water ought to be regulated were not underſtood, the uſe of the remedy muſt have been extremely hazardous: and the fatal conſequences of its improper application, we can eaſily believe to have prevented its gaining any ground on the continent, or having been adopted in Britain. Theſe laws are now, I hope, aſcertained by ample experience, and practitioners will I truſt, find themſelves directed in ſafety in the future uſe of this powerful remedy.

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* I borrow this term (catenation) from Dr. Darwin.—— It is employed to expreſs a number of actions linked together, nearly in the ſenſe of *aſſociation*: the *ſympathies*, as they are uſually called, are included under this term, and many *aſſociated* actions to which the word ſympathy has not been applied, as the connexion between the heart and the lungs, the ſtomach and the heart, &c. See Zoonomia, vol. i. ſect. xvii.

The affuſion of tepid water in febrile diſeaſes was not unknown to the ancients, though ſeldom employed by them.† It is I apprehend new in modern practice, and will be found an important addition to the liſt of our remedies. The effects differ conſiderably from immerſion in the tepid bath, as will be more fully explained in ſpeaking of that remedy. Though the affuſion of tepid water was little in uſe as a remedy, it was familiarly practiſed as a part of their daily regimen, by the Greeks and Aſiatics in the earlier periods of their hiſtory. That the Greeks in latter times, and after them the Romans, immerſed themſelves in the baths to which they were ſo much devoted, is certain†; but in the heroic age of Greece, the affuſion of tepid water ſeems to have been the mode in which this luxury was enjoyed. Abundant proofs of this are to be found in Homer, particularly in the Odyſſey, that admirable record of ancient manners. Venus, after her public diſgrace, is deſcribed as flying to the groves of Paphos, where ſhe is *laved* by the Graces, and the improvement derived to her beauty is recorded in a ſtrain

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* I mention this diſſertation on the authority of Dr. Cullen.

† Some traces of its uſe in fever are to be found in Celſus. "Quidam ex antiquioribus medicis, Cleophantus, in hoc genere morborum, (tertian intermittents) multo ante acceſſionem caput ægri multa calida aqua perfundebat, deinde vinum "dabat. Quod, quamvis pleraque ejus viri præcepta ſecutus

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"eſt Aſclepiades, recte tamen præteriit, eſt enim anceps. "*Celſus, lib.* iii. *cap.* xiv." This uſe of the tepid or warm affuſion is altogether different from what I propoſe, and was not only, as Celſus ſuppoſes, a doubtful, but probably an injurious practice. For by chilling the body it would diſpoſe it to the acceſſion of the paroxyſm. The uſe of the tepid affuſion as now propoſed, may fairly be conſidered as a new practice.

† The baths in the luxurious days of Rome were conducted at a great expenſe and, formed a very complicated ſyſtem.

ſtrain of poetry worthy of the ſubject. (*Odys. lib.* viii. *l.* 362 *to* 367.) Ulyſſes is repreſented as *laved* by attendant nymphs in the court of Alcinoüs, and in the iſland of Circe the whole proceſs of heating the water and pouring it over the naked hero is particularly deſcribed.* On this occaſion alſo, nymphs adminiſtered to him, who after the ablution anointed him all over with oil; a ſervice well calculated in every reſpect to increaſe the pleaſure and to diminiſh the riſque of the tepid affuſion.

The Romans under their Emperors, carried the ſyſtem of bathing to a height of luxury and expenſe which it never reached in Greece or Aſia, as the ruins of their magnificent *Balnea* prove to this day; and the affuſion of warm water was one of the methods by which they diverſified this favourite gratification.†

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* *Odys. lib.* x. *l.* 358, &c. compare this with the bathing of Telemachus in *lib.* xvii. *l.* 85, &c.

† See *Hieronymi Mercurialis de Arte Gymnaſt: lib.* i. *p.* 44 and 45.

CHAP. XI.

Of the uſe of cold water as a drink in fever.

TO conclude the account of my experience of the uſe of cold water in fever, it will be neceſſary for me to ſay ſomething of its effects when ſwallowed, on the ſtomach, and through it on the ſyſtem at large. Among the ancients the internal uſe of cold water in ardent fevers is recommended by Hippocrates, Galen, Celſus, and moſt of the celebrated phyſicians whoſe works have come down to us: among the moderns, that extraordinary man, Cardanus, wrote a diſſertation in its favour; and to paſs over a multitude of inferior names, Hoffman, though with ſome reſtrictions, recommends it, not in fever only, but in various other diſeaſes*. In our own country it was propoſed as an almoſt univerſal remedy by Smith, and a treatiſe has been written on it under the title of *Febrifugum Magnum*, by Dr. Hancock. In Spain and in Italy the uſe of cold water in fevers obtained in the beginning of this century a greater and more general reputation than in any other of the countries in Europe, and at one time, ſeems to have ſuperceded the uſe of all other diet as well as of medicine. This treatment was celebrated under the title of *Dieta Aquea*, and an account of it may be found in the 36th volume of the Philoſophical Tranſactions by Dr. Cyrillus, a Profeſſor at Naples, to which the reader is referred. Beſides the internal uſe of cold water, he mentions the advantage of laying powdered ice or ſnow on the bodies of the ſick.

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* *Hoffmanni Opera*, *vol.* i. *p.* 479.

Nevertheleſs the propriety of giving cold water in fevers, has been diſputed by men of high character, and particularly by the celebrated Boerhaave. His doctrine of a lentor in the blood led him to inſiſt on the uſe of warm drink and the danger of cold; and his commentator Van Sweiten, though he allows cold drink in ſome inſtances, yet in general argues againſt it.* Theſe learned theoriſts prevailed in their day over the voice of nature and the precepts of Hippocrates, and Hoffman.—In the writings of Pringle, Cleghorn, and Lind, we find little or nothing on the ſubject, though they expreſsly wrote on fevers; Dr. Cullen mentions cold drink, but gives no opinion on the propriety of its being uſed, and certainly did not recommend it in practice. He was even doubtful of the extent to which cold air might be admitted*. On the whole it may be aſſerted that the uſe of cold drink in fever is contrary to modern practice, and that where it is occaſionally given, it is adminiſtered with caution, and rather permitted than enjoined.

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* See *Boerhaavii Aphoriſm. Sect.* 743. with the commentary of *Van Sweiten.*

It is not however to the doctrine and precepts of Boerhaave alone, that the diſuſe of cold drink in fevers is to be imputed. The propriety of giving it freely has been at all times controverted, not on the ground of theory only, but from experience of the dangerous and ſometimes ſuddenly fatal effects of large draughts of cold liquids, various inſtances of which have been recorded from the earlieſt periods of medical hiſtory. While therefore ſome phyſicians have prohibited the uſe of cold drink in fevers altogether, thoſe who have recommended it from experience of its ſalutary effects, have introduced various cautions as to its exhibition, founded on certain theories, generally fallacious, on the manner in which its deleterious influence is produced. To detail the various opinions that have prevailed on this curious and important point, would be to add another chapter to the ample records of human errors. That the danger ariſing from cold drink depended on the great difference between the temperature of the liquid and of the body, and that it is therefore to be prohibited when the heat of the body is very great, is an opinion very generally received by the moderns; and among the ancients, though their doctrines were leſs erroneous, yet while the means of aſcertaining the real heat of the living body and the changes it undergoes, were unknown, it cannot be expected that they ſhould have arrived at the truth on this important ſubject.

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* See his firſt lines—cure of fever.

The effects of cold water as a drink in fevers I was naturally led to examine by my experience of its effects as an external application. I have made this examination with the thermometer in my hand, and with all the attention in my power; and the following reſults, which will ſave the reader the fatigue of reading the particulars of various caſes and experiments, ſeem to me to contain all the information neceſſary to direct our practice.

1. Cold water is not to be uſed as a drink in the cold ſtage of the paroxyſm of fever, however urgent the thirſt. Taken at ſuch times it increaſes the chillneſs and torpor of the ſurface and extremities, and produces a ſenſe of coldneſs in the ſtomach, augments the oppreſſion on the præcordia, and renders the pulſe more frequent and more feeble. Its effects in all theſe reſpects are ſimilar to

the affuſion of cold water on the ſkin in the ſame ſtage of the paroxyſm as deſcribed in pages 16 and 35, though inferior in degree. If the thirſt is gratified in the cold ſtage of the paroxyſm, it ought to be with warm liquids.

2. When the hot ſtage is fairly formed, and the ſurface is dry and burning, cold water may be drank with the utmoſt freedom. Large draughts of cold liquid at this period, are highly grateful; they generally diminiſh the heat of the ſurface ſeveral degrees, and they lower the frequency of the pulſe. When they are attended with theſe ſalutary effects, perſpiration and ſleep commonly follow.—Theſe effects are ſimilar to thoſe produced by the affuſion of cold water on the ſurface, as already deſcribed, but inferior in degree alſo. Though various caſes are on record of the paroxyſm of fever being diſſolved by cold water, uſed in this ſtage of the diſeaſe, my experience does not furniſh me with any inſtance of this kind*. Indeed ſince I became acquainted with the extraordinary efficacy of the

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* *See Alpinus. Med. Meth. lib.* ii. *cap.* 3. Where after reciting the effects of cold drink in diminiſhing heat and thirſt, and exciting profuſe perſpirations and large diſcharges of urine, he concludes, *Mirabile eſt, quomodo tale præſidium, has febres expugnet; nam execretionibus, quas aqua ſuſcitat, hae febres finiuntur.* According to the ſame author this practice was followed by the Egyptians.—*See lib.* ii. *cap.* 15.

affuſion of cold water on the ſurface, I have not truſted the ſolution of the paroxyſm to its internal uſe. I have however employed cold drink when neceſſary as an auxiliary. Throughout the hot ſtage of the paroxyſm cold water may be ſafely drank, and *more freely in proportion as the heat is farther advanced beyond the natural ſtandard.* It may even be drank in the beginning of the ſweating ſtage, though more ſparingly. Its cautious uſe at this time will promote the flow of the perſpiration, which after it has commenced ſeems often to be retarded by a freſh increaſe of animal heat. A draught of cold water taken under ſuch circumſtances will often reduce the heat to the ſtandard at which perſpiration flows more freely, and thus bring the paroxyſm to a ſpeedier iſſue.

3. But after the perſpiration has become general and profuſe, the uſe of cold drink is ſtrictly to be forbidden. At this time, I have perceived in more than one inſtance, an inconſiderate draught of cold water, produce a ſudden chillneſs both on the ſurface and at the ſtomach, with great ſenſe of debility, and much oppreſſion and irregularity of reſpiration. At ſuch times, on applying the thermometer to the ſurface, the heat has been found ſuddenly and greatly reduced. The proper remedy is to apply a bladder filled with water, heated from 110° to 120°,

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the ſcrobiculus cordis,* and to adminiſter ſmall and frequent doſes of tincture of opium, as recommended by Dr. Ruſh. By theſe means the heat is ſpeedily reſtored.

This effect of cold water uſed as a drink during profuſe perſpiration is preciſely analogous to the affuſion of it at ſuch times on the ſurface of the body, a practice known to be of the utmoſt danger, and enumerated by Hoffman among the cauſes of ſudden death. *Inveniuntur in ampliſſimo regno naturæ plura quæ brevi tempore nocent ac perimunt, ut aqua frigida corpori ſudore diffluenti immoderatius ſuperingeſta. Vol.* i. *p.* 194.

The pernicious effects of cold water applied internally and externally under profuſe perſpiration, depend on the ſame cauſes, namely, that perſpiration itſelf is a cooling proceſs, under which the heat of the body, whatever its actual ſtate may be, is ſinking; that under ſuch circumſtances, we find as a matter of fact, it parts with its remaining heat more eaſily; and on the ſudden application of cold, that this heat ſinks to a degree which diſturbs, and ſometimes

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* See the Appendix, N°. II. p. 27.

The pernicious effects of cold drink during profuſe perſpirations was known to the ancient.—*See Celſus, lib.* i. *cap.* 3.

times wholly interrupts the catenations on which life immediately depends.

Thus then we may ſafely adopt the ſame general rules for the uſe of cold water in fever as a drink, that have already been laid down for its external application. It may be uſed as a drink at any time, *when there is no ſenſe of chillneſs preſent, when the heat of the ſurface is ſteadily above what is natural, and when there is no general or profuſe perſpiration.**

Though we have arrived at theſe concluſions, the reader is entreated to believe, that the effects of cold water uſed as a drink have not been inferred from its external application, but made the ſubject of ſeparate inquiry. Yet, that in the one caſe, and in the other, they ſhould be ſimilar in kind, though different in degree, will be expected by every one acquainted with the laws of the animal œconomy, and particularly with the ſympathy that ſubſiſts between the ſtomach and the ſurface.

I have only to add, that in our common contagious fever, when I have uſed the affuſion of cold water, I have ſeldom found it neceſſary to employ it largely as a drink, and my experience of its effects

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* See p. 16.

when drank in large quantities, has been chiefly confined to thoſe caſes where the fears or prejudices of the patients or their friends, have prevented our having recourſe to the more powerful method of affuſion. For however burning the thirſt may be, it is ſpeedily abated, and even removed, with very little drink, and often without any, by the ſucceſsful uſe of the affuſion on the ſurface. Though the affuſion in general ſuffices in *our* contagious fever, yet in the uſe of cold water in the dreadful fever of Philadelphia and the Weſt Indies, it is probable that its internal and external uſe ſhould be combined; a point that muſt be determined by the actual heat of the patients, meaſured by the Thermometer, and by their ſenſation of heat, circumſtances of which it is to be regretted that we have as yet no accurate information.

CHAP. XII.

Of the diſeaſe that ariſes from drinking cold liquids after ſevere exerciſe.

IT is here natural to inquire how far the fatal effects proceeding from drinking cold water, not in fever, but in caſes where the ſyſtem has been extremely heated by bodily exertions (of which the records of medicine afford ſo many inſtances) are to be explained on the principles already laid down. If they are explicable on theſe principles, we ought to be able to ſhew, that they have occurred in ſituations where the ſyſtem, after having been much heated and enfeebled by ſevere exertions, is loſing its præternatural heat from profuſe perſpiration, and in general alſo from the ceſſation of the exertions by which this heat was originally produced. Here two powerful cauſes combine to cool the body, and if under their operation, a ſudden application of cold is made either to the ſtomach or the ſurface, the living power will, we know, reſiſt it faintly, and the fatal conſequences be accounted for.

1. In my own experience this ſudden death has occurred once only, and that many years ago. It was in the caſe of a young man who had been engaged a long time in a moſt ſevere match at fives. After it was over he ſat down on the ground, panting for breath, and covered with profuſe perſpiration. In this ſtate he called to a ſervant to bring him a pitcher of cold water juſt drawn from a pump in ſight. He held it in his hand for ſome minutes, but put it to his head as ſoon as he had recovered his breath, and drank a large quantity at once. He laid his hand on his ſtomach, and bent forwards; his countenance became pale, his breath laborious, and in a few minutes he expired. Various methods were employed to reſtore him, but in vain.

2. The following caſe reſembles very exactly that juſt given. *Blaſius, Senenſis, familiaris noſter et condiſcipulus, dum longiusculo tempore ſub ardentiſſimo ſole pilæ luſu incaluiſſet, nec* ſudore adhuc aut fatigatione remiſſis, *in ſubterraneum locum ubi vinaria erat cellula, deſcendiſſet, frigidimiſſimi vini calicem hauſit: quo epoto, ſtatim deficit.* Benevent. cap. 17. De abditis.

3. *Elegans & optimæ ſtaturæ juvenis Romanus, cum pilâ luderet,* et ſudore reſperſus, ac totus madidus, & fatigatus *ad puteum, pro ſiti arcenda veniſſet,*

exhauſtâ frigidâ recens per caldarium extractâ, ilico in terram cecidit & obiit.

4. *Alterum novimus ex iis, qui in campo negotiantur, qui quum non minus corporis totius* adapertis poris, madidus *domum reverteretur, cyathum frigidioris aquæ ebibit, et mortuus fuit.* Anat. Luſit. curat. med. cent. 2. curat. 62.

5. Forreſtus relates, that in the year 1544, Valerius Cordus, a young man of great learning and talents, went during the heat of the dog-days, to collect plants among the Florentine mountains. Exhauſted with fatigue and thirſt, he incautiouſly drank of a cold ſpring which iſſued out of one of the hills, and was immediately ſeized with a fever, of which he died; but the ſymptoms of which he has not recorded. *P. Foreſtus, lib.* i. *Scholio ad obs.* 13.

6. Scaliger relates the caſe of a reaper, who, ſtooping down to drink at a fountain after ſevere labour, inſtantly expired. *Scaliger de Subt. ad Cardan. exer.* 13.

7. In Heiſters obſervations, a caſe is related of a young man, who, about the Chriſtmas ſeaſon, had been playing and dancing at a Miln with ſome young women, and had eaten greedily of ſome hot

buttered cake. After this, being extremely thirſty, he took a large draught of ſome cold water mixed with ſnow. An inflammation in the ſtomach followed, terminating in mortification, of which he died. *Heiſter's Med. &c. Obſervations, tranſlated by Wiſeman, p.* 17.

8. *Villanum quendam nobis familiarem novimus, qui meſſis tempore, anno* 1597, *exhauſtus viribus, et totus ſitibundus præ nimio solis ardore, domum rediens, cum in magna quantitate, ad ſitim explendam, gelidam (aquam) bibiſſet, exanimatus mox juxta puteum cecidit, ac intra tres horas animam expiravit.* Georgius Graſſeccius in Theatro Anatomico.

Theſe relations are chiefly taken from the collection of Schenck—they might be greatly enlarged, and I have collected a number of ſimilar caſes; which it were a tedious and an uſeleſs taſk to detail. In all of the caſes which I have conſulted, as well as in thoſe I have related, three circumſtances are either expreſſed or may be clearly inferred—1. The body had been previouſly heated beyond the temperature of health, by exerciſe carried to fatigue. 2. To this violent exertion a ſtate of reſt had in general ſucceeded. 3. A profuſe perſpiration had taken place. So far our reaſoning is ſupported; but as theſe points are of the utmoſt conſequence in explaining the operation of cold on the human body, and as direct experiments are attended with extreme hazard, the reader will excuſe me, if I attempt to ſupport them farther, by ſuch evidence as hiſtory may incidentally afford.

1. In Quintus Curtius, (*lib.* vii. *cap.* 5.) an account is given of the march of the army of Alexander the Great in purſuit of Beſſus, through the country of the Sogdiani, which is repreſented as deſtitute of water, ſterile, and covered with ſcorching ſands. The intolerable heat, fatigue, and thirſt of the ſoldiers in their march through this burning deſert, are deſcribed with all the florid eloquence of the hiſtorian. At length fainting under their toils, they reached the banks of the river Oxus, where by indulging in large draughts of the ſtream, Alexander loſt a greater number of his troops than in any of his battles. *Sed qui intemperantius hauſerant, intercluſo ſpiritu extincti ſunt; multoque major horum numerus fuit, quam ullo amiſerat prælio.**

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* The whole particulars of this march as deſcribed by Quintus Curtius, are very intereſting. The deſert, which contained not a drop of water, was four hundred Stadia acroſs—*(per quadringenta ſtadia ne modicus quidem humor exſiſtit,)* that is, upwards of forty-ſix Engliſh miles. They began their journey in the night, directing their courſe by the ſtars, and for ſome time their march was tolerable, being refreſhed by

2. A ſimilar ſtory is related by Appian—*Appianus Alexandrinus de bellis civilibus, lib.* v. *tradidit Cornificianos milites a Pompeianis pugna fatigatos & æſtuantes, fontanam aquam avidè bibentes, ex iis plurimos emortuos. Marcel. Donat. lib.* iv. *cap.* 6. *Hiſt. med. mirab.*

3. A diſaſter of the ſame kind is recorded to have

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the dews of night, and the coolneſs of the dawn; but when the ſun roſe, the heat became troubleſome, and as the day proceeded, moſt oppreſſive; it was equally painful to ſtand ſtill or proceed. After a day of dreadful fatigue, the van-guard of the army, and Alexander himſelf, reached the Oxus towards evening; and ſuch as were themſelves refreſhed, were employed in carrying water back to the fainting troops behind. As they arrived in ſucceſſion on the banks of the river, it may eaſily be ſuppoſed that they drank without moderation, and hence the deſtruction that enſued. It was on this occaſion that Alexander diſplayed his magnanimity in refuſing the cup of water brought to him as he advanced, becauſe it was not ſufficient both for him and his companions; and that he gave a proof of his genius by ordering fires to be kindled on the high banks of the Oxus, not merely to direct his way-worn ſoldiers through the darkneſs, but to animate their fainting exertions by a proſpect of the end of their toils. Plutarch alludes to this ſtory, but does not relate it fully. I fear it is not to be found in Arrian. A ſimilar diſtreſs is mentioned by him (*lib.* vi. *p.* 425) to have occured in Alexander's march through the deſerts of the country of the Gedroſi, who inhabited the ſouthern part of the Perſian empire on the ſhores of the Indian ocean.

have occurred to the Chriſtian army in the holy wars. *Gulielmus Tyrius, lib.* iii. *c.* 16. *ſcribit, Chriſtianum agmen Piſidiam ingreſſum, regionem arentem & inaquoſam, tandem invento fluvio; quod quidem, qui largius aquam frigidam ingurgitaverunt, ſitis diſcrimen evadentes, mortem in aquarum opulentia reperere.*

Theſe hiſtorical relations ſupport very fully the doctrine I have already laid down. The cautious reaſoner may not, on a haſty conſideration, be inclined to reſt with much confidence on this ſort of evidence; but on reflection he will ſee that it is entitled to conſiderable authority; becauſe the facts are in their nature not liable to be miſtaken, and becauſe they are not likely to be miſrepreſented. It is alſo entitled to much weight, becauſe it is not given by theſe hiſtorians in ſupport of any particular doctrine; and becauſe the experiment having been made on ſuch numbers of perſons at once, it acquires an authority hardly to be aſcribed to ſolitary caſes, however accurately detailed. Theſe conſiderations induce me to lay much more ſtreſs on evidence of this kind, than on the precepts reſpecting the effects of cold drink to be found in medical authors, ancient or modern. Nevertheleſs I have looked into the greater part of the ancients on this point, (for among the moderns there is not much on the ſubject) and have found nothing, that fully conſidered, invalidates the concluſions I have laid down.

Of the ancient phyſicians the moſt copious on the uſe of water in all its forms, is Galen. He not only uſed cold drink, but immerſion in the cold bath, in burning fevers, with extraordinary ſucceſs. His relations appear to me, in general tedious and obſcure, but not deſtitute of truth; and the wearineſs of peruſing him is occaſionally relieved by the pleaſure of reſcuing a fact that was buried under maſſes of falſe theory. The reader who would conſult him on this ſubject may uſe the references below.*

In the firſt volume of *Medical Inquiries* and *Obſervations*, publiſhed by Dr. Ruſh of Philadelphia, 1789, an account is given of the "Diſorder "occaſioned by drinking cold water in warm wea"ther," which frequently occurs there. "Three "circumſtances," he obſerves, "generally concur "to produce diſeaſe or death from drinking cold "water. 1. The patient is extremely warm. 2. "The water is exremely cold. And 3. A large "quantity of it is ſuddenly taken into the body. "The danger from drinking cold water is always in "proportion to the degrees of combination which "occur in the three circumſtances that have been

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* Vol. i. 23. B.
Vol. ii. 78. C.
Vol. vii. 70. A. and forwards throughout the volume. I quote from the Latin edition in folio, publiſhed at Venice, 1656.

" mentioned." *p.* 151. Dr. Ruſh goes on to ſtate the ſymptoms of this diſeaſe, which are, I apprehend, given with accuracy. His method of cure is in my judgment proper and judicious. " I know " but one certain remedy for this diſeaſe, and that " is *liquid laudanum.* The doſes of it, as in other " caſes of ſpaſm, ſhould be proportioned to the violence of the diſeaſe. From a tea-ſpoonful to near " a table-ſpoonful has been given in ſome inſtances " before relief has been obtained. Where the " powers of life appear to be ſuddenly ſuſpended, " the ſame remedies ſhould be uſed which have " been ſo ſucceſsfully employed in recovering per- " ſons ſuppoſed to be dead from drowning." To this I would add the application of a bladder filled with water heated to 110° or 115° of Fah^t. to the pit of the ſtomach, from which I have ſeen powerful effects in reſtoring the vital heat*. But while I accede to Dr. Ruſh's practice I cannot ſubſcribe to his notion of the cauſes of this diſeaſe, or to the method of prevention founded on this notion.

Dr. Ruſh ſeems to entertain the popular opinion on this ſubject: *the body is extremely warm; the water extremely cold, and a large quantity is introduced ſuddenly*

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* See Appendix, N°. II. p. 31, 32.

ſuddenly. He apprehends the danger to ariſe from the great difference between the temperature of the body and of the water taken in. As a means of prevention he therefore propoſes to ſuch as cannot be reſtrained from drinking cold water when præternaturally heated—1. To graſp the veſſel out of which they are about to drink for a minute or longer with both hands; that a portion of heat may be abſtracted from the body and imparted to the cold liquor. 2. If they are not furniſhed with a veſſel to drink out of, but obliged to drink at a pump or a ſpring, always to waſh their hands and face previouſly to drinking, with a little of the cold water. "By receiving," ſays he, "the ſhock "of the water firſt on thoſe parts of the body, a "portion of its heat is conveyed away, and the "vital parts are defended from the action of "the cold."

The fact however is in my mind perfectly eſtabliſhed, that there is no ſituation in which the application of cold to the body, whether to the ſurface or the ſtomach, is ſo ſafe, or in general ſo ſalutary, as when the heat of the body, from whatever cauſe, is præternaturally great, provided that the body is not already in a ſtate in which it is rapidly parting with this heat; and where the body is præternaturally heated, the degree to which cold water may be drank, may be always decided by the ſteadineſs of the ſenſation of heat, and the tenacity with which the præternatural heat is actually retained. Thus in continued fevers it may be drank to a greater extent, than in the hot ſtage of intermittents, becauſe the heat is more firmly retained; the profuſe perſpiration not being at hand, by which the febrile heat of intermittents is carried off. The ancients who gave cold drink largely in continued fevers, were doubtful of its uſe in intermittents.* It may however be given (as I have already ſtated) with great ſafety in intermittents, provided it be taken in the time that intervenes after the hot ſtage of the paroxyſm is fairly eſtabliſhed, and before the perſpiration that follows it, has become general and profuſe. As however it is only in that interval that it can be given in intermittents with advantage or ſafety, we can eaſily underſtand that the ill effects ariſing from its being accidentally drank in the cold or the ſweating ſtage of the paroxyſm, (in both of which the thirſt often demands liquids) may have produced the doubts which ſome have expreſſed in regard to its uſe in intermittents, and the interdiction which in ſuch caſes, others have pronounced againſt it. We may explain alſo from the ſame conſiderations, why in the accounts that have been handed down to us of injurious effects from the uſe of cold drink in fevers, the greater part of the caſes have been intermittents.

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* See Sennertus, lib. ii. cap. ix. p. 54. Itaque Græci auctores jubent in ſtatu, cum febris acuta, ſitis, inquietudo, cordis & arteriarum pulſatio eſt vehementiſſima, ægerque avidiſſime eam expetit, aquam frigidam copioſe exhibere.—

* * *

In intermittentibus febribus vero aquæ frigidæ potus nunquam convenit.

The inſtances however that are recorded of the fatal effects of large draughts of cold liquids, have more frequently occurred after ſevere exerciſe and fatigue, than even in intermittent fever. The cauſe of this is obvious—the heat præternaturally accumulated by exerciſe, is held with leſs tenacity than even the heat in intermittents. It is diſſipated by the perſpirations that exerciſe occaſions, and is ſpeedily loſt, when to profuſe perſpiration is added a ſtate of reſt. It is then that a large draught of cold liquid is eſpecially dangerous. But while the præternatural heat is ſuſtained by continued exertion, cold liquids may be taken in moderate quantities without producing any injurious effects. They may even I apprehend be drank copiouſly without producing ſuddenly the fatal effects already deſcribed—but in copious draughts they are found oppreſſive to the ſtomach during exerciſe, and they excite languor, nauſea, and ſometimes vomiting, as

I have had occaſion to obſerve. In the narrative already mentioned of the march of Alexander's army through the deſert country of the Sogdiani, it is related by the hiſtorian, that a few of the ſoldiers, by the advice of the natives, had provided themſelves with water, of which under their burning thirſt they drank immoderately. The conſequence was that they became heavy, feeble, and unable to ſupport their arms, and this ſtate of oppreſſion was ſucceeded by ſevere vomiting. *Graves deinde avide hauſto humore, non ſuſtinere arma; non ingredi poterant; et feliciores videbantur, quos aqua defecerat, quum ipſi ſine modo infuſam vomitu cogerentur egerere. Q. Curtius, lib.* vii. *cap.* 5. The water thus waſted, or worſe than waſted, might have been uſed to advantage in wetting, from time to time, the garment next the ſkin. Thus the oppreſſion of its weight on the ſtomach would have been prevented, and the ſurface of the body being kept cool by conſtant evaporation, the heat of the ſyſtem would have been moderated, and the thirſt alleviated.

If this account of the circumſtances under which cold drink after ſevere exerciſe proves injurious, be juſt, the precautions of Dr. Ruſh to thoſe who *will* drink in ſuch circumſtances, are founded on error. By abſtracting a part of the præternatural heat of the body before drinking, the danger is not diminiſhed,

niſhed, but greatly increaſed. This enlightened phyſician will excuſe theſe obſervations, drawn from me by a conſideration of the importance of the ſubject, but accompanied by ſentiments of ſincere eſteem and reſpect.

If the effects of cold water uſed internally under ſevere exerciſe, are not entirely analogous to the effects produced by its affuſion on the ſkin;—the difference will be eaſily underſtood, by thoſe who conſider, that where a quantity of water is ſwallowed, beſides the influence of the cold, the ſtomach ſuſtains a load, from the weight and the bulk of the liquid, particularly oppreſſive under conſtant action and agitation of the voluntary muſcles, from which the ſurface moiſtened with water is entirely free; and on the other hand, that the evaporation from the ſurface promoted by the immediate acceſs of the external air, muſt operate more directly in cooling the body, and particularly in counteracting the burning rays of the ſun, than water taken into the ſtomach. With theſe exceptions, the operation of cold liquids on the ſtomach and on the ſurface of the body are analogous in the caſe of præternatural heat produced by bodily exertion, as in all other caſes of præternatural heat. As it is ſafe to drink cold water in proportion as the heat from exerciſe is great and ſteady, ſo alſo is it ſafe according to this ratio, to pour it on the ſurface, or to immerſe the body in the cold bath.

In the earlier ſtages of exerciſe, before profuſe perſpiration has diſſipated the heat, and fatigue debilitated the living power, nothing is more ſafe, according to my experience, than the cold bath. This is ſo true, that I have for ſome years conſtantly directed infirm perſons to uſe ſuch a degree of exerciſe before immerſion, as may produce ſome increaſed action of the vaſcular ſyſtem, with ſome increaſe of heat; and thus ſecure a force of re-action under the ſhock, which otherwiſe might not always take place. The popular opinion that it is ſafeſt to go perfectly cool into the water, is founded on erroneous notions, and ſometimes productive of injurious conſequences. Thus, perſons heated and beginning to perſpire, often think it neceſſary to wait on the edge of the bath until they are perfectly cooled; and then plunging into the water, feel a ſudden chillneſs that is alarming and dangerous. In ſuch caſes the injury is generally imputed to going into the water too warm, whereas in truth it ariſes from going in too cold.*

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* Dr. A. Munro Drummond, in his inaugural diſſertation, "*De Febribus Arcendis*," the only ſpecimen left of his admirable talents, ſpeaking of the effects of the cold bath as a preventative againſt the action of contagion, obſerves, "Nec frigida protinus fugienda vel calida temperanda, quamvis cutis pallida aliquantiſper fiat vel leviter aliquis inhorruerit. Si exercitatio antecedat quæ citra laſſitudinem & ſudorem eſt, hæc facile evitari poſſe experiendo didici: et fere, ſi nil aliud obſtet, quo ante quiſque plus incalueret quam aquam intraverit, eo calidior emerget.

But though it be perfectly ſafe to go into the cold bath in the earlier ſtages of exerciſe, nothing is more dangerous than this practice after exerciſe has produced profuſe perſpirarion, and terminated in languor and fatigue. Becauſe, as has already been repeated more than once, in ſuch circumſtances, the heat is not only ſinking rapidly, but the ſyſtem parts more eaſily with the portion that remains.

This account of the operation of the cold bath will explain ſome circumſtances very generally mentioned by writers on the effects of cold on the human body, and hitherto not properly accounted for. That the Roman youth in the heat of their exerciſe in the *Campus Martius*, frequently plunged into the Tyber, is a fact univerſally known; they found in this practice a high enjoyment, and they believed it conducive to health, and more eſpecially to ſleep. On the other hand various relations may be cited of the injurious effects of an apparently ſimilar practice; the moſt remarkable of which is the caſe of Alexander the Great, when covered with duſt and ſweat, he threw himſelf into the Cydnus, and was ſeized with a diſeaſe, of which he nearly periſhed; one of the beſt authenticated facts in ancient hiſtory.

In the diſſertation *De febribus arcendis*, by Dr.

A. Munro Drummond, already quoted, theſe facts are brought together in the following words.—*Alexander, quondam diei fervidiſſimo tempore, liquore fluminis invitatus, vix dum in Cydnum amnem deſcenderat, cum gravi inde morbo implicitus eſt. Romana juventus, poſt quotidianas in Campo Martio exercitationes, pulvere ſimul & ſudore perfuſa, Tyberi impune laſſitudinem curſus natandi labore depoſuit. Mali nimirum adſuetudine duramur in his ſicut in cæteris rebus omnibus. Theſ. Med. vol.* iii. *p.* 154. Doubtleſs the influence of habit has a conſiderable ſhare in regulating the effects of cold on the human body; but the circumſtances juſt mentioned ſeem capable of an explanation on other principles.

On the *Campus Martius*, the exerciſes of the Roman youth were carried on with all the vehemence of emulation. Swimming formed a part of thoſe exerciſes*, and generally terminated the foot-race. The youthful candidates in this exerciſe directed their courſe towards the banks of the river, and plunged headlong into the ſtream. Sometimes the contention did not terminate till the river was once, or perhaps twice ſwam acroſs. Hence it will eaſily be ſeen, that they were accuſtomed to immerſe themſelves in the water in the very fervor of their exertions, when the heat was præternaturally great; and not after the body was cooled by profuſe perſpirations, or exhauſted by long continued fatigue. In this ſituation the practice was ſafe; without taking into conſideration, that the perſons concerned were in the flower of life, fortified by early habits, and partly defended from the ſhock of immerſion by the inunctions which ſeem to have been generally uſed among the Romans, before the cold, and after the hot bath; and which were particularly

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* See *Horace, lib.* i. *Ode* 8. Where the poet, after queſtioning Lydia why her lover neglects his accuſtomed exerciſes on the *Campus Martius*, among other particulars aſks,

Cur timet flavum Tiberim tangere?

See *Vigetius, lib.* i. *cap.* x.

See alſo *Heiron. Mercur. lib.* iii. *cap.* 14.

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* See *Horace, Satyr, lib.* ii. *Sat.* 1.

——————Ter uncti
Tranſnanto Tiberim, ſommo quibus eſt opus alto.

The ſubject of the ancient unguents is treated of by *Hieronymus mercurialis, de arte Gymnaſtica, lib.* i. *cap.* x.

The ſame author, *(lib.* iii. *cap.* 14*)* ſpeaking of ſwimming ſo much in eſtimation among the ancients, obſerves, *quo pacto vero ſimilis exercitatio perageretur, tanquam res vulgatiſſima ſilentio praeterita fuit fere ab omnibus, uno excepto Antyllo, qui nataturos prius moderate unxiſſe, et frictione corpora praecalfeciſſe deinde e veſtigio in aquam ſe projicere debere voluit, quaſi omnes ſanitatis gratia natantes eas regulas obſervarent.*

This method of previouſly anointing the body, might be recommended to our modern ſwimmers, not only as defending them from the ſhock of immerſion, but as enabling them to glide through the water with leſs effort.

ticularly employed by the *athletæ* of Greece and Rome in all their exerciſes. It was the more ſafe, becauſe the ſtream of the *yellow Tiber* being comparatively ſcanty and ſlow, its waters ſpeedily received the influence of the ſun, and acquired the temperature of the atmoſphere. Where the air and the water are of the ſame temperature, the rarer element prepares the body for the contact of that which is more denſe.

The circumſtances under which Alexander plunged into the Cydnus, were different in many eſſential points. He had marched at the head of his troops to ſeize a paſs in Mount Taurus, neceſſary to facilitate his paſſage into Cilicia. After having ſecured his object, he deſcended from his elevated ſtation, through a road, difficult and full of defiles, to the city of Tarſus, which was ſituated at the bottom of the mountains. His whole march, which probably continued ſeveral days, was attended by extraordinary exertion, not from the nature of the country only, but alſo from the preſſure of circumſtances. In aſcending the heights, he had to haſten forwards, leaſt the enemy ſhould pre-occupy the paſs by which Cilicia muſt be entered. In deſcending from the immenſe elevation he had reached, he was ſtimulated by the deſire of ſaving the city of Tarſus, the capital of the province, which the Perſians threatened to conſume by fire. In both of theſe enterprizes Alexander was ſucceſsful. It was at the cloſe of this extraordinary march, according to the teſtimony of all the hiſtorians, that the conqueror, advancing into Tarſus at the head of his troops, covered with duſt and ſweat, and exhauſted with long-continued toil, ſtripped himſelf in the ſight of his army, and plunged into the pure and cold waters of the Cydnus, which ran through the city. The ſymptoms that followed, are deſcribed with ſufficient minuteneſs and preciſion. *Vixque ingreſſi ſubito horrore artus rigere cœperunt: pallor deinde ſuffuſns eſt, et totum propemodum corpus vitalis calor reliquit. Exſpiranti ſimilem miniſtri manu excipiunt, nec ſatis compotem mentis in tabernaculum deferunt. Q. Curtii, lib.* iii. *cap.* v. From the length and difficulty of the march, it is natural to ſuppoſe that Alexander muſt have been cooled as well as debilitated by extraordinary perſpiration and fatigue, and under ſuch circumſtances, immerſion in the cold and rapid Cydnus, was followed by the conſequences which we ſhould expect from the principles already laid down.*

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* If an objection be urged againſt our laying much weight on a relation taken from a period of hiſtory ſo remote, I reply, that I am far from reſting any part of my concluſion upon it, having the firmer foundation of actual obſervation. But as this adventure of Alexander is mentioned by almoſt every writer on the effects of cold on the living ſyſtem, to exemplify

In his eſſay on ſwimming, Franklin makes the following obſervation: "During the great heats of "ſummer there is no danger in bathing, however "warm we may be, in rivers which have been "thoroughly warmed by the ſun. But to throw "ourſelves into cold ſpring water, when the body "has

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the danger of its being applied when the body is præternaturally heated, it ſeemed proper to ſhew, that it is capable of a more juſt and natural explanation.

In regard however to the fact itſelf, of Alexander having ſuffered from bathing in the Cydnus, it might be eaſily ſhewn, that we cannot reject it without denying entirely the weight of hiſtorical evidence. 1. This exploit was performed before a hoſt of witneſſes, by ſome of whom it is recorded, and it is mentioned by all the hiſtorians. 2. The circumſtances that preceded it, and the conſequences that followed it, are alſo recorded with great minuteneſs, and form a natural chain of events. 3. The act itſelf of ſtripping before his army, and ſpringing into the Cydnus, may have an air of improbability; but is not ſo inconſiſtent with Greek, as with modern manners; and however unlike the dignity of great commanders in our own days, is perfectly characteriſtic of Alexander—of that ardent and daring temper, and of that buoyant imagination, which were equally regardleſs of danger and decorum; a temperament, that joined to extraordinary talents, attached his ſoldiers to him in ſo ſingular a degree, and which in the æra of the world in which he lived, was calculated to carry him to the aſtoniſhing elevation which he attained. 4. The exploit itſelf was of a nature not likely to be feigned; and the iſſue of it had nothing marvellous—there was nothing in any part

“ has been heated by exerciſe in the ſun, is an im-
“ prudence which may prove fatal. I once knew an
“ inſtance of four young men, who having worked
“ at harveſt in the heat of the day, with a view of
“ refreſhing themſelves, plunged into a ſpring of
“ cold water; two died on the ſpot, a third the
“ next

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of it, by which the ſon of Ammon, or his followers, could be in any way gratified.

The effects of Alexander's bathing in the Cydnus, gave a celebrity to this river which it otherwiſe would not have acquired. I have called it cold and rapid, meaning to ſpeak of it comparatively with the Tiber. The Cydnus was one of the principle ſtreams that deſcended from Mount Taurus, (*Strab.* xiv. *p.* 462.) at the bottom of which ſtood the city of Tarſus. From the great elevation of the mountain, the Cydnus muſt in this ſituation have been rapid and cold; and colder becauſe rapid. From the ſame cauſe it would be eaſy to ſhew (if it were at all to the purpoſe) that the tranſparancy aſcribed to its waters was probably aſcribed juſtly. Quintus Curtius imputes its coldneſs to the rays of the ſun being excluded by the ſhadyneſs of its banks: the more accurate and philoſophic Arrian, mentions as the cauſe, the loftineſs of the mountains whence its fountains deſcended. Quintus Curtius ſpeaks of it as gliding through a gentle deſcent, *(leni tractu e fontibus labens,)* and Tibullus of its ſilent and placid ſtream.

At te, Cydne, canam, tacitis qui leniter undis,
Cæruleus placidis per vada ſerpis aquis.

Strabo, on the other hand, (*lib.* xiv. 392.) mentions its current as moſt rapid, and this to ſome of the ſcholiaſts ſeems a contradiction. It is not ſo in reality. If the declivity was uniform, the current might be at once ſwift and

" next morning, and the fourth recovered with " great difficulty. A copious draught of cold wa- " ter in the ſame circumſtances, is often attended " with the ſame effect in North America." The authority of the American Bacon is of great weight in medicine, as in every branch of ſcience; and particularly in what reſpects immerſion in water; for doubtleſs he ſpent more time in this element than any philoſopher of modern days. It may however be eaſily ſuppoſed that he adopted the commonly received opinion, that the injury aroſe from the perſons in queſtion going in *when hot*, inſtead of from going in *when cooling, after having been heated*; to which laſt circumſtance, it can hardly be doubted, that the fatal accident he relates, was to be imputed. It is not however to be denied, that the difference between the temperature of ſpring and river water, at certain

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ſilent. The river Moffat (the ſource of the Annan) deſcends from the mountain of Hartfell, by a declination, which in three thouſand yards, amounts to two hundred and thirty feet, with ſuch uniformity, that in the courſe of ſome miles there is hardly a ſingle ripple. To finiſh a digreſſion by far too long, but which may relieve the dryneſs of a profeſſional ſubject, it may be obſerved, that our account of the illneſs of Alexander receives confirmation from a difference among the hiſtorians reſpecting its cauſes; ſome imputing it to his having bathed in the Cydnus, and others to his previous fatigue: at the diſtance of two thouſand two hundred years, it may be clearly determined that both circumſtances combined in producing his diſeaſe.

certain ſeaſons of the year, will make a great difference (in any circumſtances of the body) in the conſequences of immerſion. Springs which iſſue copiouſly from the earth, are, as we know both from reaſoning and obſervation, nearly of the temperature of the earth at ſome diſtance under the ſurface; and this temperature, when unaffected by chemical changes, is probably (for actual experiment is not yet ſufficiently extenſive to affirm the propoſition univerſally) of the mean heat of the atmoſphere, taking the year round, under which it lies.* On the other hand, rivers even of the largeſt ſize, acquire at a certain diſtance from their ſource, the temperature of the earth, and riſe towards the end of ſummer, to nearly the higheſt degree of the ſummer heat.†

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* See Dr John Hunter's valuable paper on this ſubject, in the Philoſophical Tranſactions for 1788, *p.* 53.

† Thus the Merſey at Liverpool, in the middle of the tide, is in the month of Auguſt from 65° to 69°. On the 20th of Auguſt 1795, it roſe in the middle of the tide to 70°, and under the rocks at High Park, the mercury ſtood on the 21ſt, at high water, at 72°.—In the air, at the ſame time, it was at 77°.—The weather had been uncommonly warm for fourteen days before, and 72° was probably the average heat of that fortnight. It ſeems to be this average that the tide of the Merſey acquires. Obſervations made in our different æſtuaries every week, would give the actual and the comparative heat of the ſeaſons (on the coaſt at leaſt) with much more accuracy than obſervations in the atmoſphere.

In countries where the temperature of the ſeaſons has little variation, the difference between the heat of the rivers and ſprings will be very inconſiderable. That this is the caſe in Jamaica, we know from the experiments of Dr. Hunter; but in Pennſylvania, to which it is probable that Dr Franklin's obſervations more particularly refer, this difference at certain ſeaſons of the year muſt be very great, the difference of the temperature of the ſeaſons being ſo very conſiderable. The rivers of Pennſylvania in the latter end of ſummer, are probably nearly as warm as the Buxton bath, or from 78° to 82°; and in the Buxton bath there is reaſon to believe, that even after conſiderable fatigue, the chill of immerſion is too inconſiderable to produce much hazard. The ſprings of Pennſylvania, and more eſpecially the deep ſunk wells, if they retain the ſubterranean temperature (the medium heat of the ſeaſons) will probably be in the heat of ſummer and autumn, twenty or twenty-five degrees cooler than the rivers or the atmoſphere*; a difference capable of producing, in the ſituation of the body which renders the application of cold dangerous, the moſt ſerious effects. It is doubtleſs owing to the great comparative coldneſs of their ſprings and wells, that fatal accidents from drinking cold water, and bathing in cold ſprings, are ſo frequent in Pennſylvania. In the Weſt Indies, where water is ſeldom to be procured below 77°, ſuch accidents, if they ever occur, are very rare.

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* The wells at New York are from 54° to 56°, according to Dr. Routh. *See Philoſophical Tranſactions for* 1788, *p.* 61.

I ſhall cloſe my remarks on the circumſtances which render the cold bath ſafe or hazardous after exerciſe, with the following narrative :—

On the firſt of September, 1778, two ſtudents of medicine at Edinburgh, ſet out on foot on a journey, a conſiderable part of which lay along one of the rivers of Scotland. They ſtarted by ſunriſe, and proceeded with alacrity in the cool of the morning. At the end of eight miles, they breakfaſted, reſted for an hour, and then reſumed their journey. The day grew warm as it advanced, and after a march of eight miles more, they arrived heated, but not fatigued, on the banks of the river abovementioned, about eleven in the forenoon.—Urged by the fervor of the day, and tempted by the beauty of the ſtream, they ſtripped inſtantly, and threw themſelves into the river—The utmoſt refreſhment followed, and when they retired to the neighbouring inn, this was ſucceeded by a diſpoſition to ſleep, which they indulged. In the afternoon they proceeded, and travelling ſixteen miles farther at a ſingle ſtretch, arrived at the inn where they were to ſleep, a little after ſun-ſet.—The afternoon had been warm, and they perſpired profuſely; but the evening was temperate and rather cool. They had travelled for ſome miles ſlowly, and arrived at the end of their journey, ſtiffened and wearied with their exerciſe.

The refreſhment which they had experienced in the morning from bathing, tempted however one of them to repeat the experiment, and he deſcended perfectly cool into the ſame river, expecting to relax his limbs in the water, and afterwards to enjoy profound ſleep. The conſequences were very different. The Tweed, which was ſo refreſhing in the morning, now felt extraordinarily cold; and he left the water haſtily. No genial glow ſucceeded, but a feveriſh chill remained for ſome time, with ſmall frequent pulſe, and flying pains over the body. Warm liquids and frictions brought on at length conſiderable heat, and towards morning perſpiration and ſleep followed. Next day about noon they proceeded on foot, but the traveller who had bathed was extremely feeble; and though they had to perform a journey of a ſingle ſtage only, yet as ſome part of it was difficult and mountainous, he was obliged to take the aſſiſtance of a carriage which overtook them on the road. It was ſeveral days before he recovered his uſual vigour. This relation will not I hope be deemed of the leſs authority, becauſe it is given by the perſon who ſuffered by his imprudence*. It is unneceſſary to point out the application of theſe incidents to the doctrines already laid down.

It ſeems to be a general truth, that from whatever cauſe the heat of the body is increaſed, in proportion to this increaſe (provided no local diſeaſe has occurred) is the ſafety with which cold may be applied. In the celebrated experiments of Dr. Fordyce, Sir Charles Blagden, and others, of which an account is given in the Tranſactions of the Royal Society, *vol.* lxv. *p.* 111, and 484, it is repeatedly mentioned, that theſe gentlemen paſſed from a room heated to 200°. and upwards, into the cold air, with perfect ſafety. " During the whole day," ſays Sir Charles Blagden, " we paſſed out of the heated room,

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* Thoſe who know the road from Edinburgh to Moffat will be at no loſs to mark the different ſtages of this adventure.

I may here obſerve, that ſimilar illneſſes to the above occur frequently, from the imprudence of the country people, (the Boltoneers, as they are called) who make an annual viſit to the river below Liverpool, for the purpoſe of bathing.—In ſuch caſes, I have found the diſeaſe that followed, was not inflammatory, as might be ſupppoſed, but rather a fever of debility, as in the inſtance detailed.

(where the temperature of the air ſeems to have been 240°, and ſometimes 260°) "after every experiment, immediately into the cold air, without "any precaution; after expoſing our naked bodies "to the heat, and ſweating moſt violently, we inſtantly went into a cold room, and ſtaid there, "even ſome minutes before we began to dreſs; yet "no one received the leaſt injury." *Ibid. p.* 494.

The freedom from injury, and even from inconvenience, which theſe gentlemen experienced, depended on the increaſed heat of the body, and the increaſed action of the arterial ſyſtem.—Had they continued expoſed naked to the cold air till the heat ſunk as low as its natural ſtandard, and the heart and arteries ſubſided into their uſual ſtate of action, their ſituation would have been very hazardous.

In ſimilar experiments repeated at Liverpool, of which a detail is given by Dr. Dobſon, in the ſame volume of the Philoſophical Tranſactions, *p.* 463, the gentlemen concerned, paſſed from the heated room into the cold air, with equal impunity. My friend Mr. Park aſſures me, that after remaining ſome time in the ſtove, where the heat was as high as 202°, he went into the external air without a great coat, or any other than his uſual clothing, during a hard froſt, and perceived neither injury nor inconvenience.

In many manufactures, the perſons employed are expoſed to extraordinary degrees of heat, particularly in the manufacture of glaſs. Such perſons, guided by nature, often endeavour to obtain relief, by expoſing themſelves to degrees of cold, which on the commonly received opinions, would in their ſituation appear extremely dangerous. Thus at Glaſgow in the ſpring of the year 1780, I learnt, that it was common for the workmen in the glaſs manufactory, after enduring for ſome time the conſuming heat of their furnaces, to plunge into the Clyde; a practice which they found in no reſpect injurious.

A great variety of the phenomena reſpecting the influence of cold on the living body, receive an eaſy explanation by attending to the principle already mentioned. Thus, after the heat of the body is increaſed two or three degrees in the hot bath, it is not only ſafe, but refreſhing, to plunge into the cold bath, as I have repeatedly experienced. A practice of this kind prevails, as is well known, in Ruſſia, where it is common, after remaining ſome time in the hot bath, to roll naked in the ſnow, returning to the warm bath as before. The Ruſſian hot baths vary from 106½° to 116° of Fahᵗ.* and I find by experiment that the actual heat of the living body is increaſed in the hot bath (unleſs under particular circumſtances afterwards to be explained) when the temperature is no higher than 100°. We may therefore ſafely conclude, that in making theſe ſingular tranſitions, the heat of the Ruſſian is in the firſt inſtance increaſed beyond the natural ſtandard; and it is to this increaſe that the ſafety, as well as the agreeableneſs of the practice is to be attributed.

It is in this way that we muſt account for the ſafety of a practice which prevailed at Rome under the firſt of the emperors. After the overthrow of the republic, the Romans conſoled themſelves for the loſs of their freedom, by a more unbounded indulgence than ever in thoſe ſenſual gratifications which had led to their fall. Of theſe, the pleaſures of the bath formed a diſtinguiſhed part, they ſought every means of heightening and diverſifying them, and connected them with other epicurean enjoyments. The mild and

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* See *Diſſert. phyſica experimentalis de calore Animalium*, by *J. A. Braun*, in the *Nov. Comment. Academiæ Scientiarum Imperialis Petropolitanæ*, vol. xiii. The Abbé Chappe D'Auteroche ſays, the Ruſſian baths are as high as 60° of Reaumer, (160° of Fahrenheit) *Philoſophical Tranſactions*, *vol.* lxv. *p.* 112, but this is incredible, unleſs he ſpeaks of vapour baths.

tepid immerſion of the Greeks no longer ſatisfied them; they heated their baths to the utmoſt pitch of endurance; and as they roſe reeking from their ſurface, veſſels full of cold water were daſhed over their naked bodies, as a high gratification in itſelf, and a means of ſtimulating the ſenſes to gratifications ſtill higher. Such practices could not however be continued without injury: In the bagnios of *Imperial* Rome, the firm republican melted down into a ſlave; the powers of his mind became enfeebled, the vigour of his frame decayed, and he loſt for ever the bold impreſſions of freedom and virtue*. It

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* This ſubject might lead to digreſſions, which my limited plan does not admit. I ſhall content myſelf with a few obſervations. Though private baths were in uſe in Rome from the earlieſt periods of their hiſtory, yet the ſyſtem of public hot baths did not, it is underſtood, commence till the days of Auguſtus. According to Dion, Mæcenas introduced it.—Agrippa followed his example—It was ſoon carried to an aſtoniſhing height, and the conſtruction of baths where the people might be accommodated *gratis*, was an eſtabliſhed and a ſucceſsful method of gaining their affections. The extraordinary expenſe and magnificence of thoſe ſtructures are well known—the remains of the baths of Caracalla and Diocleſian, teſtify their grandeur in our own days. According to Fabricius, there were 856 public baths at Rome, and ſome of theſe were large enough to contain at once 1800 perſons. The *rage* for hot bathing in Rome, under the firſt Emperors, exceeded all bounds.

In regard to the exact temperature of their baths, as the thermometer was not known to the ancients, we cannot ſpeak preciſely. We have however the authority of Seneca and Plutarch for ſaying, that the bath of the Greeks was of a

It was ſuppoſed by Dr. Cullen, who paid much attention to the operation of temperature on the living ſyſtem, that in all ſudden changes from a higher to a lower temperature, a ſenſation of cold takes place, even though the loweſt point of change be ſuch as when permanent, becomes oppreſſive by its heat. Thus that a change of the external air from 90° to 85°, is accompanied by a ſenſation of cold, though this ſpeedily goes off, and is ſucceeded by a ſenſation of heat, if the temperature of 85°

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moderate temperature; and as Alexander found refreſhment in the bath in the burning fever of which he died, it probably did not exceed 93°, or at moſt 96°, of Fahrenheit. But at Rome, in the days of Seneca, the hotteſt baths were moſt in eſtimation, and thoſe of Nero ſeem to have exceeded all others in heat—We may infer this from one of the beſt of Martial's epigrams.

Si temperari balneum cupis fervens
Fauſtine, quod vix Julianus intraret,
Roga, lavetur, Rhetorem Sabineum,
Neronianas is refrigerat thermas. *Lib.* iii. 25.

A perſon was employed at one time to regulate the heat of the baths, but in Seneca's days this had fallen into diſuſe. (*Epiſt.* 57.) This rage for bathing was checked by Adrian, and regulated by Severus. The faſhion of heating the baths to this extraordinary degree did not continue—We have the authority of Galen, who flouriſhed ſoon after Seneca and Plutarch, that in his days very hot baths were no longer in uſe. *See Hier. Mercur. lib.* i. *cap.* 10.

be continued[†]. This obſervation ought, I think, to be reſtricted to degrees of heat inferior to that of the human body, for it does not ſeem true when applied to ſuperior degrees of heat. If, for inſtance, a perſon is immerſed in the bath heated to 104°, and a quantity of cold water be ſpeedily admitted, ſo as to lower it to 98°, a ſenſation of a very agreeable nature takes place, not however to be denominated cold—Where the actual heat of the body has been raiſed, and the circulation accelerated, a much greater reduction of the temperature of the bath muſt take place before it becomes cold to the ſenſations. In the ſame manner a perſon under the ſteady heat of fever, (102° to 106°) ſuddenly throwing off his bed-clothes, and expoſing himſelf naked to the external air, does not perceive it cold, but merely cool, a ſenſation too which goes ſpeedily off. It is true there are ſome exceptions to this in fever, of which I have given one inſtance in detail. *(See page* 46.*)*

The manner in which our ſenſations are affected by changes of temperature, is a ſubject of importance, as well as of difficulty, for without a more preciſe knowledge of this, the action of temperature on life cannot be underſtood. In the ſtate of health, I believe, however, that our ſenſations of heat and cold may be reduced to general principles.—But in certain of the Neuroſes, as well as of the Pyrexiæ, theſe ſenſations are much perverted, and even in health they are much influenced by conſtitution, habit, and ſituation. To enlarge on this point would however lead into the general ſubject of the operation of cold on the living ſyſtem, for which I am not ſufficiently prepared, and which I ſtudiouſly avoid.

[†] See *Diſſert. Med. Inaug. De frigore*, by A. Cullen, publiſhed at Edinburgh 1780, (*p.* 8 and 9) which contains the beſt general view of the doctrines of the author's celebrated father on the operations of cold.

CHAP. XIII.

Uſe of the cold Bath in convulſive Diſeaſes—In Inſanity—General Remarks.

IT might now be expected that ſome obſervations ſhould be offered on the operation of cold water applied to the ſurface, or the ſtomach, in the various caſes that have been related; but beſides that it is adviſable that the reader ſhould form his concluſions in a great meaſure for himſelf, we ſhall be better prepared for ſuch general reaſonings as may ſuggeſt themſelves, when we have obſerved the operation of the cold bath on convulſive affections, and on other diſeaſes, claſſed by Dr. Darwin under the ſame general title, the *diſeaſes of volition**.—To ſave repetition, the reader is here ſuppoſed to have peruſed the paper in the Appendix, N°. I, of which he will conſider the following obſervations as forming a ſequel.

Since the original publication of this paper, I have ſeen three different caſes of tetanus ariſing from wounds, in which the affuſion of cold water was employed alone, and in all of theſe the diſeaſe terminated fatally. I have ſeen a fourth caſe in which the affuſion of cold water was conjoined with the liberal uſe of wine and opium, where the patient recovered. And I have ſeen two other caſes in which the cure was truſted to wine and opium alone, in one of which the iſſue was fortunate, in the other unfortunate.

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* The third claſs of Dr. Darwin.—*See Zoonomia, vol.* ii.

It is however proper to obſerve in the three firſt caſes, the diſeaſe was far advanced, having reſiſted other powerful remedies before the patients fell under my care; and that as the power of diglutition was nearly loſt, the affuſion of cold water was employed under deſperate circumſtances, as a *dernier* reſort. It was beſides impoſſible to uſe this remedy to any extent, for the powers of life were too much exhauſted to ſuſtain the continued application, or the frequent repetition, of ſo violent a ſtimulus.—There is alſo in the advanced ſtages of tetanus, an objection to the uſe of a remedy that requires much change of poſture. In ſuch caſes, every exertion of the will on the voluntary muſcles has a tendency to induce a general convulſion.—From a conſideration of all theſe circumſtances, and from more extenſive experience, I ſhould now be diſinclined to the uſe of the cold bath in any of its forms, in the ſymptomatic tetanus, unleſs in the earlier ſtages of the diſeaſe, when the vigour is leſs impaired, and the convulſive actions leſs firmly catenated.

The practice, firſt introduced by Dr. Ruſh, of giving wine in large quantities in tetanus, of which a ſucceſsful caſe is detailed in the paper already mentioned*, ſeems to me to be fully confirmed, as the moſt efficacious treatment of this very dangerous diſeaſe. The wine ought however to be combined with opium in conſiderably larger doſes, according to my later experience, than in the caſe already referred to.

In the Appendix, N°. I, ſeveral caſes are given in which the cold bath was uſed in other convulſive diſorders. This remedy I have now employed in various diſeaſes of this claſs for fourteen years, and on the whole, it has been attended with conſiderable ſucceſs.—One concluſion, of rather a ſingular nature, was drawn from thoſe caſes, which my ſubſequent experience has uniformly corroborated :—“ That the efficacy of the cold bath in convulſive diſorders, is much promoted by its being employed during the preſence of convulſion*;” or, as I would now expreſs it, *that the chief benefit derived from the cold bath in convulſive diſeaſes, depends on its being uſed in the paroxyſm of convulſion; that its efficacy conſiſts in reſolving or abating the paroxyſm; and that when this effect is produced, the return of the paroxyſm is greatly retarded, if not entirely prevented.* To this ſingular fact, for which I am unable to account, I invite the attention of phyſiologiſts: it may throw ſome light on the laws of aſſociation; a ſubject which Dr. Darwin has ſhewn to be of ſuch vaſt importance in the living ſyſtem, and which hitherto has been ſo little underſtood. The following caſe, taken from the records of our Infirmary practice, is in many reſpects curious, and will ſerve to corroborate the poſition juſt laid down.

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* See Appendix, N°. I. *p*. 9.

John Weſtmore, aged 22, was admitted into the Infirmary on the 11th of October 1792. In conſequence of a fright, he had been ſeized with fits two months before, which now recurred ſeveral times every day, of various duration, from two minutes to an hour. During theſe his conſciouſneſs was wholly aboliſhed. Theſe fits occurred without warning, and were peculiar in their appearance. At firſt the muſcles of one ſide were ſtrongly retracted, then thoſe of the other alternately; and then the muſcles of both ſides acting together, the whole trunk of the body was drawn upwards to the head; this action reſembling very exactly that of the victim of the law, ſuſpended in the agonies of death. His faculties of mind did not ſeem as yet impaired, nor the animal functions much diſturbed. No medicines were ordered for this patient, but a bucket-full of cold water was directed to be thrown over him, the inſtant of the acceſſion of the fit. Some circumſtances prevented this being done for a few days, during which he went into the cold bath daily, when the fits were off him, and with ſeeming advantage; the number of paroxyſms being reduced from eight or ten in the twenty-four hours, to two or three. At length he was thrown into the cold bath on the inſtant of the acceſſion of one of his fits, which was ſpeedily terminated, and from this time forth he had no return for fourteen days, when he was diſcharged as cured. Weſtmore continued free of complaint till the beginning of February following, when his fits returned. He was re-admitted on the 24th of that month, and at this time his fits recurred ſix or ſeven times in the day. He was again directed to use the cold bath daily—and to have a bucket of water thrown over him on the acceſſion of every fit. By this practice he ſpeedily recovered, and ſince that time the diſeaſe has never returned. He is now, *(May* 1797*)* in perfect health.

I have uſed the cold bath during the paroxyſm of epilepſy in various inſtances, but in general without the patient being rouſed to conſciouſneſs or ſenſation, and without advantage. In one caſe of this diſeaſe, which occurred in the hoſpital practice, the paroxyſm returned periodically every afternoon. In this inſtance a cure was affected, by applying a cataplaſm, formed chiefly of tobacco, to the ſcrobiculus cordis, about half an hour before the expected return, by which a powerful impreſſion on the ſyſtem was produced, and the paroxyſm of epilepſy prevented. This practice repeated ſeveral days at the expected periods, deſtroyed the deſeaſed catenation, and the cure was permanent.—In this mode of application, the peculiar effects of tobacco on the nervous ſyſtem is ſpeedily produced, and it is perhaps the ſafeſt way of exciting them; becauſe the application can be diſcontinued at pleaſure; an advantage of great importance in the uſe of ſo deleterious a medicine, and which cannot be obtained when it is thrown into the ſtomach or rectum. I was induced to uſe tobacco on this occaſion, from having obſerved it to ſucceed in the cure of obſtinate intermittents, when applied in the ſame manner, previous to the expected acceſſion of the paroxyſm. It is however an agent ſo powerful and ſo debilitating, that I do not willingly employ it; and it is obvious that it can only be uſed in convulſive diſeaſes, when their returns are periodical.

In ſpaſmodic diſeaſes which riſe into general convulſions, I have ſeen theſe convulſions rendered leſs frequent, and for a time ſeemingly diminiſhed in violence, by medicines uſed during the intervals; but ſeldom effectually cured, except by means uſed during the act of convulſion, at the inſtant of its acceſſion, or at the period of its expected return. Of the medicines employed during the intervals of epilepſy, the oxyd of zinc ſeems to me to have ſome efficacy; but the digitalis purpurea ſtill more. —In conſequence of its powerful influence, I have uſed this laſt medicine in many caſes of epilepſy, both in private and hoſpital practice; of twelve of which I have preſerved particular regiſters. *In every one of theſe*, it diminiſhed the number of paroxyſms, and the intervals were ſo much lengthened, that in ſeveral inſtances we entertained hopes of a cure. It did not however finally ſucceed in any one of them; and when, as the conſtitution became accuſtomed to its influence, the fits began to return, and we increaſed the doſe of the medicine, its deleterious qualities, ſo well deſcribed by Dr. Withering, preſented a danger greater than that of the diſeaſe, and obliged us to abandon it. On one occaſion, we had an opportunity of obſerving, that puſhing this medicine to the utmoſt extremity did not effect a cure. A female patient took it ſo raſhly as to produce the utmoſt danger of her life. Her pulſe ſunk to thirty-two in the minute, her heat at the axilla to 89°;—her ſight failed, her reſpiration became laborious, and inceſſant vomitings threatened immediate diſſolution. She was however recovered by the warm bath and opiate clyſters, but as her ſtrength was reſtored the epileptic paroxyſms returned.

In the Appendix, N°. I, *p.* 17, I have mentioned the connexion between convulſive diſeaſes and inſanity, and conjectured that the ſame principles are in many caſes applicable to the treatment of both. This connexion has been ſince explained, and the ſubject very happily illuſtrated by Dr. Darwin, *(Zoonomia, vol.* ii. *c.* 3.*)* who has arranged the whole of theſe diſeaſes under one claſs, and thrown a new light on one of the moſt curious and obſcure parts of pathology. The following caſe taken from the regiſter of our Aſylum, may ſerve as a new proof of this connexion.

June 2, 1796. L. I. aged 32, a man of very irregular habits of life, was admitted into the Aſylum, in a ſtate of furious inſanity. His diſeaſe was ſuppoſed to have been brought on by exceſſive drinking. As caſes of inſanity, including the whole method of cure, are ſeldom recorded, I will give this in detail.

Very powerful methods of coercion were required ed in the firſt inſtance, and the bowels were afterwards cleared by a ſaline purgative. At night he took eighty drops of laudanum, which produced imperfect ſleep, and in the morning following he was more wild than ever.

Having in ſome caſes of inſanity, found very extraordinary effects from ſuch doſes of opium as induced profound ſleep, I directed that five grains of this medicine ſhould be given every four hours, till ſleep ſucceeded. Next day (4th *June*) it was reported to me, that he had taken twenty-ſeven grains of opium in the twenty-four hours. From the firſt five grains there was no apparent effect; after the ſecond, he ſlept an hour and a half; after the third, three hours. Though the medicine had been twice repeated in doſes of ſix grains each, he had had no return of ſleep—He appeared wild, agitated, and feeble, rather in a ſtate of alarm than of fury. His pulſe 100 and ſoft, with gentle and general perſpiration.

It ſeemed not prudent to puſh the medicine farther, but having often in caſes of this nature obſerved the ſoporific effects of opium to come on, many hours after the medicine had been diſcontinued, he was directed to have milk as his food, and to abſtain from medicines of every kind for twenty-four hours.

The report of the 5th is as follows: "He remained in a perpetual ſtate of alarm till eight o'clock laſt night; after that hour he ſlept, but not ſoundly. He has taken nine pints of milk in all, having been very thirſty. It agreed with him till ten this forenoon, when he vomited conſiderably. Since that time he has been more tranquil and feeble." In this ſtate, I directed all medicines to be diſcontinued, and ordered him a pint of wine, with two pints of gruel daily, and ſuch ſolid food as his ſtomach could receive and digeſt. This plan was continued till the 17th, when his ſtrength was conſiderably reſtored, but his inſanity as violent as ever. It was now directed that he ſhould go daily into the bath heated to 95°, and that he ſhould take half an ounce of the infuſion of the digitalis (according to the form of Dr. Withering) thrice a day.

He continued on this plan with little variation till the 29th, the report of that day is as follows: "Till the 25th the digitalis produced no obvious effect. On that day ſickneſs came on and his pulſe was hardly perceptible. The digitalis was therefore omitted. He has been regularly in the bath, and while in it, was once ſeized with *diliquium animi*, but the mental derangement did not afterwards abate. His pulſe is now about 94, feeble and irregular.—He is a good deal emaciated, but more fierce than ever; he talks inceſſantly, with the utmoſt incoherence,

rence, and the aſſociation of his ideas ſeems wholly perverted."

In this ſtate I ordered the digitalis and the tepid bath to be diſcontinued, as well as all other medicine. I directed a nutritious diet, but without wine or opium, and placed him in a ſituation to be as little as poſſible expoſed to external impreſſions. For three days there was little change in his ſituation—his appetite improved, and he perſpired much. At the end of this period, he had ſuddenly a lucid interval of ſeveral hours duration, but before I could witneſs it, he was again become furious.

At this time (the 16th *July*) I directed that he ſhould take three drachms of peruvian bark, half a drachm of vitriolated iron, and the ſame quantity of aromatic powder, in the form of an electuary, daily; and that twice every day, he ſhould be ſubjected to the tepid affuſion for a minute or two at a time, the water being poured gently over him of the heat of 88°.

The following is the report of the 16th *July*: " The tepid affuſion was continued till the 14th, but without any tranquillizing effect; the other medicines have been given regularly. On the 9th he became calm and rational: and ſlept the following night for eleven hours profoundly; but ſtarted ſuddenly from this ſleep, as wild and furious as ever. This ſtate of mind continued twenty-four hours, when he again became tranquil, and continued ſo till he went to bed. He ſlept a few hours quietly, but awoke as before, under extreme agitation, and continued fierce and raving for twelve hours and upwards. Since this time, he has had an almoſt regular ſucceſſion of paroxyſms of inſanity, and of tranquillity of mind, which divide his time pretty equally. In general, he goes to bed tranquil, and ſleeps quietly till about three in the morning, when he awakes under the horror of ſome frightful dream; and this introduces his inſanity and fury, which continue from ten to fourteen hours."

In this ſituation I ordered an emetic to be adminiſtered to him late in the evening, in hopes that it might interrupt this beginning catenation. Being diſappointed, I had again recourſe to an opiate at bed-time; and this alſo proving inefficacious, I directed that he ſhould be awoke before the expected acceſſion, and kept awake beyond its period of attack. By theſe means we diſturbed the regularity of the alternations, but without any advantage; on the contrary, his lucid intervals became ſhorter in duration, and leſs complete.

Perplexed with theſe extremes, and keeping in mind the ſucceſs of the cold bath in convulſive diſeaſes, I ordered it to be tried on the preſent occaſion. The inſanity returning with great violence on the 21ſt, he was thrown headlong into the cold bath—He came out calm, and nearly rational, and this interval of reaſon continued for twenty-four hours. The ſame practice was directed to be repeated, as often as the ſtate of inſanity recurred.

The following is the report of the 30th: "The direction has been followed, and on the morning of the 23d, he was again thrown into the cold bath in the height of his fury, as before. As he came out, he was thrown in again, and this was repeated five different times, till he could not leave the bath without aſſiſtance. He became perfectly calm and rational in the bath, and has remained ſo ever ſince."

This patient continued with us for ſome time afterwards, bathing every other day, and taking the oxyd of zinc in ſmall quantities. He never relapſed, and was diſcharged ſome time afterwards, in perfect health of body and mind.

It has been contended by Dr. Darwin, that as convulſive diſeaſes ariſe from inordinate action in the muſcles, ſo maniacal diſeaſes ariſe from the ſame inordinate action in the organs of ſenſe:—and as the muſcles and the organs of ſenſe are the proper inſtruments of the will, the diſeaſed actions of the one and the other are by him arranged as one claſs, under the title of *Diſeaſes of Volition.* But in the ſyſtem of Dr. Darwin, the actions of the organs of ſenſe conſtitute our ideas; a deranged action of theſe organs is therefore no other than what is uſually denominated a derangement of mind; in other words inſanity*. As therefore convulſion and madneſs, may be conſidered as the ſame diſeaſe differently ſituated; and as the ſeat of the diſeaſe may be changed from various cauſes, and extended through various ſympathies and catenations, we have thus an explanation of that connexion between convulſive affections and inſanity, and particularly of their alternations with each other, which have been obſerved in every period of medicine.—That ſeveral of the modes of treatment applicable to the one, are alſo applicable to the other, has long been known. The arnica montana, the oxyd of zinc, the digitalis purpurea, and opium, are medicines uſed equally in convulſions and in inſanity. The caſe which has juſt been related extends this analogy, and is the more ſtriking in this point of view, becauſe the ſucceſs of the cold bath was almoſt inſtantaneous, and occurred in the paroxyſm of inſanity, in the ſame manner as has been before obſerved of its operation in convulſion.

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* If, as is generally believed, the motions of the organs of ſenſe rather excite ideas than conſtitute them, the reaſoning of Dr. Darwin is not affected by this change of terms—That madneſs is a diſeaſe of the body, cannot be diſputed, whether we admit motions in the organ of ſenſe or not.

It is ſuppoſed by Dr. Darwin, that the motions excited by volition and by ſenſation, proceed in different directions; the firſt paſſing from the central parts of the ſyſtem towards the ſuperficies, the ſecond from the ſuperficies towards the central parts. Hence he concludes that they are oppoſed to, and mutually deſtructive of each other. To increaſe the force of voluntary actions muſt therefore be to leſſen poſitively, as well as relatively, the actions that ariſe from ſenſation, and the converſe of this propoſition is equally true. Whatever faith we may give to the hypotheſis of Dr. Darwin, his concluſion ſeems to be ſupported by a great variety of phenomena. That the higher degrees of volition and ſenſation are incompatible with each other, is a truth of great importance in every part of the ſcience of life, whether moral or phyſical*. It was the doctrine of another great phyſiologiſt (whoſe praiſe will now be heard with approbation, ſince he is beyond the reach of praiſe) " that no two actions can take place in the ſame conſtitution, nor in the ſame part at the ſame time;"* and though he applied this doctrine chiefly to ſhow the incompatibility of different diſeaſed actions with each other, yet it ſeems applicable alſo to explain the operation of a great part of the more powerful remedies. The poſition of Dr. Darwin reſolves itſelf into this more general principle of Mr. Hunter, and the operation of the cold bath in the paroxyſm of madneſs, or of convulſion, receives an eaſy explanation under either doctrine. We may ſay with Dr. Darwin, that the powerful ſtimulus given to ſenſation, deſtroys the previous diſeaſed volition, or in the more general and more ſimple language of Mr. Hunter, that the ſudden application of cold water to the ſurface, produces an action of the ſyſtem with which the previous diſeaſed action is incompatible. In order however to the ſucceſs of the cold bath in either of theſe ſpecies of diſeaſe, it will be readily perceived, that it is neceſſary that no conſiderable læſion of ſtructure

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* The reader who is not already acquainted with this part of Zoonomia, will not be able to underſtand how convulſions, which in common language are conſidered as involuntary, are claſſed as diſeaſes of volition. He is therefore referred to the work in queſtion for a full explanation. In the mean time we may obſerve that Dr. Darwin conſiders all mo-

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tions as voluntary which have originated in volition, though from their violence no longer ſubject to the will. Volition is uſed by him in a ſenſe ſomewhat different from its common acceptation.

* John Hunter. *Treatiſe on the Blood. Introd. p.* 3.

ſtructure ſhall have taken place, and that the morbid actions be not ſo ſtrong as to produce inſenſibility to impreſſion, and ſhall not have been ſo often repeated as to form indiſſoluble catenations. It ſeems alſo neceſſary that the diſeaſes in queſtion ſhall be ſuch as are ſuſceptible of general paroxyſms; a fact which is not ſo eaſily explained. Finally, it is neceſſary that the digeſtion ſhall not have been much impaired, or the vigour of the circulation much debilitated, leaſt the action of cold be too ſtrong for the living powers. Under theſe reſtrictions, the cold bath, according to my experience, may often be applied with advantage, and always with ſafety, in convulſive diſeaſes, and in inſanity; for though in theſe diſeaſes, even in their more violent forms, the temperature of the body is little, if at all, increaſed, yet the actual heat is retained with great tenacity, as I have found by many obſervations.

The reſiſtance which the maniac oppoſes to cold, he oppoſes alſo to contagion, and to other noxious powers, as has often been obſerved with ſurpriſe and admiration. This reſiſtance ſeems to belong to every ſpecies of increaſed voluntary exertion, where the functions of digeſtion and nutrition are not impaired. The illuſtration of this poſition would not be difficult, and the concluſions to be deduced from it are in a high degree intereſting and important; but a diſcuſſion of the ſubject would lead us too deep into the general doctrines of life.*

CHAP.

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* Dr. Marcard, of Pyrmont, in his work on the effects of warm and cold baths, entitled, *Ueber die Natur, und Gebrauch der Bader*, lately publiſhed, has quoted my paper on the effects of the cold bath in convulſive diſeaſes (Appendix No. I.) and it appears, that he does not aſſent to my concluſions. Dr. Marcard ſuppoſes indeed, that the cold bath may be uſeful in convulſive diſeaſes, where there is no local affection ſtimulating the ſyſtem, but he contends that it *muſt* be hurtful in caſes where convulſions originate in ſtimuli, as worms, or crudities in the alimentary canal, teething, &c. It does not appear to me that Dr. Marcard has underſtood my paper perfectly, and I do not feel myſelf diſpoſed to yield my experience to his ſpeculations. The doctrine which Dr. Marcard ſeems to maintain, that where one ſtimulus is already acting, the application of another muſt be injurious, is contrary to experience, and as I apprehend, to the laws of the animal œconomy. The work of Dr. Marcard is a valuable one, and deſerves to be tranſlated; but it ſeems to be too full of thoſe general and ſweeping obſervations, which though nearly baniſhed from other ſciences, continue in the writings of phyſicians, and ſerve at once as a proof and a cauſe of the imperfection of our art. I fear this little volume will not be found free of them.

CHAP. XIV.

General view of doctrines reſpecting fever.—Hippocrates—Galen, &c.—The ideas of the author reſpecting the nature of this diſeaſe.

THE great difficulty which men have in all ages experienced in the acquiſition of knowledge, has ariſen from the promptitude of the human mind to decide in regard to cauſes. To the weak and ignorant, preſumption is as natural, as doubt is intolerable, and with ſuch, belief is almoſt always a creature of the imagination. Nor is this peculiar to weakneſs and ignorance: to retain the mind unprejudiced and undecided in the inveſtigation of ſtriking and intereſting phenomena, till by the painful ſteps of induction, the hidden cauſe is revealed, is an effort of the moſt difficult kind, and requires the higheſt and rareſt powers of the underſtanding. The records of every part of ſcience, bear ample teſtimony to this truth, particularly the records of medicine, and in a ſtill more eſpecial manner that part of medicine which treats of the nature of fever. The moſt eminent phyſicians in every period of the world, impatient of obſerving and delineating, have been eager to explain and even to ſyſtematize; and the ſcience of life owes its corruptions more to the miſapplication of learning, than even to the dreams of ſuperſtition. Paſſing over the theories derived from the ſplended fictions of the Greek philoſophy, various are the falſe doctrines introduced into medicine in modern times, and from the more certain branches of ſcience. The principles of mechanics, of chemiſtry, of magnetiſm, and of electricity, have in ſucceſſion been employed to explain the motions of organic life, and have ſerved only to miſlead and to deceive.

Hippocrates who lived at too early a period to be acquainted with the collateral branches of ſcience, ſtudied life and diſeaſe in the book of nature, and had the merit of an original obſerver. Perceiving the increaſe of heat to be the moſt remarkable ſymptom in fever, he aſſumed this for the cauſe, and founded his diſtinctions of fevers, on the different degrees of the intenſeneſs of this heat. He had not an inſtrument that could meaſure this exactly, and neceſſarily truſted to his ſenſations. In forming his diagnoſis, he placed his hands on the breaſts of his patients, depending more on the degree of heat than on the ſtate of the pulſe, the nature and connexions of which he did not underſtand*. His practice appears to have been natural

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* Riollay's Introduction, &c. *p.* 9.

and judicious, and founded on his theory. He directed linen dipped in cold water to be applied to the hotteſt parts; drew blood away both by cupping-glaſſes and the lancet; and adminiſtered cold water and cooling drinks, particularly barley water and honey. This ſimple practice, will acquit the Coan ſage with the candid and reflecting, of many of the idle theories which have been imputed to him, and which doubtleſs were the offspring of after times.

In the days of Galen, philoſophy and ſcience had made ſome advances, and the corruptions which he introduced into medicine from thoſe ſources are well known. Preſerving however the doctrine of his maſter, that heat was the cauſe of fever, his practice founded on this notion, was in ſome reſpects bold and ſucceſsful, as was before obſerved. To him followed the Arabians, who ſtill retained the original doctrine of Hippocrates, but with new modifications and corruptions, by which the real nature of fever was ſtill farther obſcured.

In our own country, and in the laſt century, aroſe Sydenham. He alſo was an original obſerver, and to him medicine is doubtleſs much indebted. That he recorded ſymptoms with great accuracy; that he eſtabliſhed many important facts as to the treatment of diſeaſe, and that he was a more cautious reaſoner than his predeceſſors or contemporaries, are facts that are indiſputable. But, though he affected not to theorize, he was a theoriſt in every page of his works, and though he founded his reaſonings on what appeared to him ſelf-evident principles, yet time has proved them to be defective and fallacious. It was the poſtulate of Sydenham, that every diſeaſe is nothing elſe but an endeavour of nature to expel morbific matter of one kind or another, by which her healthy operations are impeded. In this endeavour ſhe is not to be obſtructed but aſſiſted, and the proceſs carefully watched and promoted by which ſhe accompliſhes her purpoſe. By one or other of the emunctories, this is finally effected, and till it be effected, health cannot be reſtored. Under this general notion, the inordinate actions of fever are perpetually compared to the motions of fermentation, by which nature ſeparates the vitiated particles from the blood previous to their expulſion. The doctrine of Sydenham may be traced to remote ages; under his ſanction it ſpread over Europe, and has deſcended in one form or other to our own times. Sydenham's doctrine introduced the ſyſtem of Boerhaave, (to which we have already alluded) as well as other chemical theories of leſſer fame, and the erroneous modes of treatment to which they gave birth. The inordinate heat which Hippocrates conſidered as the cauſe of fever, in the eye of the chemiſts appeared naturally enough, a neceſſary, and within certain limits, a ſalutary conſequence of the proceſs ſtirred up by nature to expel the diſeaſe !

The dawn of a juſter pathology of fever is to be found in the works of Hoffman. Though he alſo undertook to be the interpreter of nature's intentions, he contemplated her proceſs in fever with more ſagacity, and rejecting chemical and mechanical analogies, endeavoured to diſcover the cauſe of fever in the peculiar nature of the vital motions. He ſuppoſed the noxious cauſe producing fever (in the language of the ſchools the remote cauſe) to operate firſt on the living ſolids, occaſioning a general ſpaſm of the nervous and fibrous ſyſtem, beginning in the external parts and proceeding towards the centre. In conſequence of this a contraction of the veſſels of the extremities muſt of courſe take place, impelling the circulating fluids in an increaſed ratio on the heart and lungs ; which ſtimulating theſe organs to increaſed action, the fluids are thereby repelled towards the extremities, and thus the phenomena of fever are produced. There are therefore two diſtinct ſet of motions in fever, the firſt from the extremities towards the centre, ariſing immediately from the ſpaſm, and accompanied by a ſmall pulſe, anxiety and oppreſſion ; the ſecond, from the centre toward the ſurface, which is the effort of nature to reſolve this ſpaſm, and marked by a full ſtrong pulſe, and increaſed heat. The firſt of theſe ſet of motions are baneful, and ſometimes fatal; the ſecond are medicinal and ſalutary. By theſe views the phyſician is to be directed in counteracting the morbid actions, and aſſiſting the ſanative proceſs of nature*.

Since to theorize ſeems to be the inevitable propenſity of men of genius, this theory of the celebrated Hoffman, which accords with appearances, and has a conſiderable connexion with practice, may be allowed the tribute of applauſe.

The ſyſtem of Hoffman, produced that of Cullen, which may perhaps be conſidered as the prevailing doctrine at preſent, when however theory of every kind has loſt much of its weight on mature underſtandings.—According to Hoffman the firſt effect of the remote cauſe of fever is the ſpaſm, producing a re-action, as has already been mentioned. Dr. Cullen introduced a previous link into the chain—He contended that the firſt effect of the noxious effluvia (the remote cauſe) was a general debility, affecting the ſenſorium commune. To this debility he attributed the ſpaſm, and to the ſpaſm the re-action of the heart and arteries; which re-action continuing till the ſpaſm is reſolved, removes the debility and the diſeaſe. According to Hoffman the ſpaſm belongs to the claſs of motions that he denominates baneful, but Dr. Cullen preſumes it to be ſalutary, and therefore aſcribes it, in the language of the ſchools, to the *vis medicatrix naturæ*. It is not my purpoſe to appreciate theſe changes introduced into the theory of Hoffman, or to attempt to eſtimate with preciſion the Cullenian doctrine of fever, as it arranges phenomena, or applies to practice. Debility of a peculiar kind, ſpaſm, and re-action of the heart and arteries, ſeem all of them links in the chain of fever, and in the firſt lines of Dr. Cullen, the hiſtory of the diſeaſe is recorded with extraordinary minuteneſs and accuracy. I bow with reſpect and ſenſibility to the genius of this illuſtrious man; but his doctrine ſeems erroneous, in as far as it enters into the ſuppoſed intentions of nature. and defective, in paſſing over the morbid heat, and the morbid aſſociation, which form the ſucceſſive links of the chain.

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* See Hoffman's Works, vol. i. tom. ii. page 10.—*Geneva edition.*

This doctrine of fever, as well as all the other doctrines of its celebrated author, was aſſailed by Dr. Brown. Aſſuming the exiſtence of an unknown principle, as inherent in the living fibres, to which he gave the name of excitability, he explained all the phenomena of life and diſeaſe, as dependent on this principle, acted on by ſtimuli; which ſtimuli, applied in the due proportion, produce the juſt degree of excitement, that is, the ſtate of health. If the ſtimuli are diminiſhed below the healthy proportion, he ſuppoſed the excitability to accumulate; if encreaſed beyond this proportion, he ſuppoſed it to be expended; and from theſe different conditions he attempted a general theory of diſeaſe. Diſeaſes he divided into two claſſes, Sthenic and Aſthenic, or diſeaſes of increaſed and of diminiſhed excitement, in the laſt of which claſſes he placed typhus, the fever of which we treat. In the excitability, Dr. Brown admitted of no change, except in regard to quantity; in the excitement, no variation, except in regard to ſtrength; and in all univerſal diſeaſes he ſuppoſed the whole ſyſtem to be equally affected.* Having therefore aſſigned to fever its place in his ſeries of deſcending excitement, he refuſed to inquire into its ſymptoms, or to enlarge on its treatment. It is evident that ſuch a theory could embrace none of the characteriſtics of the diſeaſe. Debility, the firſt link in the chain of Dr. Cullen, formed according to Dr. Brown the eſſence of fever. The exiſtence of ſpaſm he denied, re-action he derided, and the morbid heat and morbid aſſociation, he wholly overlooked. It is not however to be diſputed that his general views of diſeaſe had a ſalutary influence on the practice in typhus, by encouraging a more liberal uſe of wine and opium, remedies of the utmoſt importance, and now univerſally adopted.

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* This was his general poſition; it is true he relaxed a little from it in his explanations.

The various modes of action of the living principle, or to uſe his peculiar language, of the ſenſorial power, which were unknown to Brown, have been obſerved and illuſtrated by the author of Zoonomia.

In the theory of fever delivered by Dr. Darwin, the influence of morbid aſſociation is very fully explained. I am not however diſpoſed to adopt this theory, which the learned and candid author himſelf conſiders as incomplete, becauſe it aſſumes the truth of his peculiar doctrines, to which it cannot be expected that a haſty aſſent ſhould be given—conſidering their vaſt extent and importance, and conſidering alſo that many other fabrics of genius of the ſame kind, have crumbled down into the ſand of of which they were formed.*

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* See Zoonomia, vol. i. p. 625, *concluſion.* Of this great work, ſo recently publiſhed, the opinion of the learned is not yet expreſſed, and it is not probable that it will be fairly appreciated in the preſent generation. Much as I admire this

Diſmiſſing therefore as far as it is poſſible, all theories from the mind, let us briefly conſider the proceſs of nature in fever, and examine what inferences may be ſafely drawn from the ſucceſſion and catenation of the leading ſymptoms, and the remedies which experience has pointed out as moſt ſucceſsful. Our obſervations muſt of courſe be conſidered as applying to the typhus, or low contagious fever of Britain, more particularly, but generally to all idiopathic fevers.

Medical ſcience has not aſcertained the various remote cauſes which may produce fever, and I avoid this inquiry on which I have nothing new or ſatisfactory to offer*. The firſt ſymptom

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offspring of profound reflection, and of ſingular ingenuity, the poſition common to Dr. Darwin and Dr. Brown, that the ſenſorial power, or excitability, is a ſubſtance which accumulates and diminiſhes in the inverſe ratio of the ſtimulation, has not my aſſent. On the contrary, it appears to me, that whatever the nature of the vital energy (the ſenſorial power or excitability) may be, it does not act in a way that admits the relations of quantity to be applied to it, and the contrary doctrine, which introduces into the the theory of Dr. Darwin ſo many epicycles, ſeems to me the mortal part of his immortal work. I am aware that Dr. Darwin holds the doctrine mentioned with certain modifications; which however do not appear to me ſatisfactory.

* In confining the remote cauſes of fever to contagious

of the diſeaſe is a ſenſe of debility or languor, very diſtinctly marked on the countenance, followed by paleneſs, cold and tremor; the functions of the mind are weakened in a correſpondent degree with thoſe of the body, and all the actions of the ſyſtem are enfeebled. This general debility is peculiar in its kind. The ſymptoms reſemble thoſe produced by inhaling certain gaſes whoſe properties are known; and this circumſtance ſerves to ſtrengthen thoſe views of the nature of contagion which the new chemiſtry has ſuggeſted. They ſeem alſo to have a reſemblance, though a more remote one, to the effects of certain poiſons operating on the ſtomach, or introduced into the circulation by the abſorbents. The remote cauſe of fever may be conſidered as itſelf a poiſon acting directly on the ſenſorium commune. Where this poiſon is peculiarly concentrated and malignant, or where the ſyſtem is much debilitated, the powers of life are ſometimes oppreſſed and extinguiſhed in the firſt ſtage

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human effluvia, and to marſh miaſmata, Dr. Cullen probably ſimplified too far. Cold, under certain circumſtances, ſeems to produce fever, and the conſtitution ſeems at times to *take on* fever from unknown and internal cauſes. The conjecture of Dr. Mitchel of New York, that the gaſeous oxyd of nitrogen is the cauſe of the dreadful fever of America and the Weſt-Indies, is highly ingenious, and deſerves great attention.

ſtage of the diſeaſe. In general however, a re-action or reſiſtance commences; the heart and lungs are rouſed into increaſed exertion by the preſſure of the accumulating fluids, and ſtrive as it were, to repel them back on the ſurface and extremities; while a ſpaſmodic, or morbid ſtricture of the extreme veſſels oppoſes the reflux of the fluids, and thus maintaining the inordinate preſſure on the centre, excites the heart, arteries, and lungs, to ſtill more violent exertions. In this conteſt the ſtomach is uſually brought into ſympathy, and nauſea and vomiting are often induced.

This conſtitutes what appears to be a ſtruggle between the living energy, and the morbid cauſe—between the power of the centre, and the reſiſtance of the extreme veſſels—in the courſe of which a degree of preternatural heat is generated, and the phenomena of the hot ſtage are produced. When the powers of life prevail, the ſtricture on the extreme veſſels, and on the exhalents of the ſkin, at length gives way, and a profuſe perſpiration being poured out on the ſurface, the heat is carried off, and the febrile ſymptoms ſubſide. Without entering into detail, this may be conſidered as a general view of the ſingle paroxyſm. It often however occurs, that the ſolution of the ſpaſm on the extreme veſſels does not take place, or is incomplete, and that the morbid heat is not carried off. In this caſe the morbid actions go on, and the diſeaſe runs into continued fever. For ſome time the ſpaſm on the extreme veſſels remains, and the heat is præternaturally great; but theſe ſymptoms do not continue through the whole courſe of the diſeaſe. On the contrary, the heat towards the latter ſtages, will ſometimes be found as low as the natural ſtandard, ſometimes lower, and the capillaries of the ſkin be completely relaxed; while the inordinate action of the heart and arteries continues, being carried on by the diſeaſed aſſociation produced in the courſe of the diſeaſe, which retains its influence in this as in other caſes, after the cauſe that produced it ceaſes to operate.

To this general view of fever, it is probable that little objection will be offered, by thoſe who are familiar with the diſeaſe. But exceptions may be taken to the language as not ſufficiently clear of theory, againſt which the author had declared. It may be ſaid, for inſtance, that in applying the active forms of ſpeech to the re-action or reſiſtance of the conſtitution, we ſeem to run into the Sthaalian doctrine, or at leaſt to aſſume the exiſtence of the *vis medicatrix naturæ* of Dr. Cullen; and that to enumerate ſpaſm as a link in the chain of fever, is in fact to admit the leading peculiarity of his ſyſtem. To this it may be replied, that there is no ſubject on which the imperfection of language is ſo much perceived, as in our attempts to deſcribe the phenomena of life. In the ſtrictneſs of ſpeech we properly employ the paſſive mood of our verbs, in recording the phenomena of inanimate matter, and confine the active mood to thoſe vital motions, which are accompanied with conſciouſneſs and deſign. But there are various actions peculiar to life, which are not accompanied by conſciouſneſs, or ſubject to the will, and which depend on laws wholly diſtinct from thoſe which regulate inanimate matter. If we borrow the phraſeology and the *mood* in which we ſpeak of inanimate matter in recording theſe actions, we are apt not only to form indiſtinct conceptions of their nature, but to apply the principles which regulate inanimate motions to the motions of life. This is an obvious, and as experience teaches us, an ample ſource of error. If, on the other hand, we employ the active forms of ſpeech, theſe immediately ſuggeſt conſciouſneſs and deſign, and the imagination forms to itſelf an ideal being, as directing theſe actions, in whom conſciouſneſs and deſign may reſide. This alſo is a natural and fruitful ſource of error.—Both theſe extremes we would avoid. But the penury of language obliges us to uſe one or the other mood in treating of the phenomena in queſtion, and on reflection it appears, that in our preſent ignorance of the firſt principles of life, a clearer notion will be formed of the nature of thoſe motions in which the whole ſyſtem ſympathizes, deſtitute though they be of conſciouſneſs and deſign, by the occaſional uſe of the active forms of ſpeech, ſince they have a much ſtricter analogy with thoſe vital actions, to which theſe forms are juſtly applicable, than with the motions of inanimate matter. Conſiderations of this kind may have influenced the late John Hunter in the uſe of his terms, which are chiefly *active*, and which, though far from perfect in themſelves, are more happy than thoſe of former phyſiologiſts, as his views of organic life were more original, more unprejudiced, and in general more accurate and profound.

Debility of a peculiar kind, is then the firſt operation of the poiſon producing fever—the neceſſary conſequence, or as ſome contend, the concomitant effect, is a ſpaſm, or contraction of the arteries; but more eſpecially of the extreme veſſels, and the capillaries of the ſurface—hence follows an accumulation of blood on the heart and lungs—the re-action of thoſe organs—the generation of morbid heat—and of morbid aſſociation. On each of theſe links in the chain of fever, let us offer a few remarks.

1. It has been aſſerted by ſome inveterate theoriſts, that the debility in fever has nothing in it peculiar. It is enough to reply, that ſimple debility often exiſts even in extreme degrees, without producing the phenomena that conſtitute fever.

2. It has been aſſerted, that a ſpaſm on the extreme veſſels does not exiſt in fever, or that if it does exiſt, it may ſafely be diſregarded. Thoſe who hold the firſt opinion, ſuppoſe the paleneſs and ſhrinking of the ſurface to ariſe merely from the enfeebled circulation, by which the blood is not propelled into the extreme parts, and doubtleſs this has its effect. In ſyncope, however, where this paleneſs and ſhrinking ariſe from pure debility, as ſoon as the heart and arteries recover their energy, the blood flows immediately back into the extreme veſſels as before. That this is not the caſe in fever, the moſt curſory obſervation will convince us. After the re-action of the heart and arteries has been ſome time eſtabliſhed, the blood is indeed propelled through the veſſels of the ſurface that circulate the undivided fluid; but the capillary arteries that ſeparate the aqueous part remain conſtricted, as well as the orifices of the ſkin that pour it out on the ſurface.

3. It is however contended, that this ſtricture may be diſregarded, ſince it is now very generally admitted, that the perſpirable matter is not excrementitious, and ſince we find that its obſtruction in other circumſtances, and from other cauſes, produces little or no injury to the conſtitution. A little reflection will ſerve to refute this poſition. Spaſm on the extreme veſſels muſt neceſſarily affect

the condition of the heart and lungs, from the increaſed preſſure of the circulating fluids on theſe organs: it muſt alſo affect the ſtomach not only from its connexion with the heart and lungs, but from its direct ſympathy with the ſurface.—Dr. Cullen has put theſe truths beyond controvery. But this is not all; the obſtruction of perſpiration—of profuſe perſpiration—in the paroxyſm of fever, obſtructs the proceſs by which the conſtitution expels the morbid heat, and thus leaves the ſyſtem under the influence of a general ſtimulus of the moſt powerful nature. It was a poſition of the celebrated Boerhaave, that the morbid heat in fever being a ſymptom only, might therefore be diſregarded. But can we ſuppoſe, that a heat ſix or ſeven degrees greater than that of the blood in health, however generated, will not have the moſt important effects on the ſyſtem, and if it ſtands in the relation of effect to the preceding ſymptoms, that it will not operate as a cauſe on thoſe which ſucceed?—Doubtleſs this morbid heat re-acts on the heart and arteries; ſtimulates the inordinate exertions by which it was excited; irritates the ſpaſm of the extreme veſſels which prevents its being expelled; and prolonging the increaſed action of the heart and arteries, eſtabliſhes a morbid aſſociation, which carries on this action after the heat itſelf has ſubſided, and the ſpaſm relaxed.

4. What then, it may be inquired, is the actual ſtate of the animal heat in the different ſtages of the paroxyſm of intermittent, as meaſured by the thermometer? In different trials it ſeemed liable to conſiderable variety, but the following is the general reſult, which however the reader will receive as ſubject to the corrections that a more extenſive experience may ſuggeſt. In the cold ſtage, the heat is diminiſhed, not on the ſurface only, as ſome have imagined, but very probably over the whole ſyſtem. I have found it under the tongue, and at the axilla, as low as 94°, 93°, and 92°, and on the extremities many degrees lower. At this time the ſtomach feels cold, and univerſal tremor takes place. Warm, and even hot liquids, are highly grateful, and the warm bath, heated to 100°, more grateful ſtill. The drinking of warm liquids ſhould be freely indulged in, and there is reaſon to believe that immerſion in the warm bath (as practiſed by Galen) would be found as ſalutary as it is grateful. By degrees the pulſe increaſes in frequency and force, and the heat begins to riſe, but with much irregularity. It doubtleſs accumulates firſt in the centre of the ſyſtem, and from the centre is puſhed along with the blood, towards the ſurface. This proceſs does not however go on in any regular progreſſion, but like almoſt all the other animal proceſſes, with alternate increaſe and relaxation. At times it appears retrograde; and even when it is ſo far advanced

that the heat taken at the axilla, and under the tongue, is greater than the ſtandard of health, a ſlight acceſſion of external cold will produce a general chillneſs, and bring back the oppreſſion on the heart and lungs. On the ſurface itſelf the reſtoration of heat takes place with no regularity.—Certain parts are heated firſt. In certain parts the heat is above what is natural, while in others, it remains below this ſtandard; and hence ariſes that mixed ſenſation of heat and cold, which every one acquainted with fever muſt have experienced in the tranſition from the cold to the hot ſtage of the paroxyſm.

The irregularity with which the heat of the ſurface is reſtored, probably ariſes from this circumſtance, that two cauſes conjoin in producing this effect—the reflux of warm blood from the centre, and the ſympathy between the ſtomach and ſkin. To this laſt is attributed the circumſcribed heat of certain parts of the ſurface, while the reſt remains cool, this local heat being excited by the increaſed action of the veſſels of theſe parts, awakened by the reſtored heat and tone of the ſtomach, which in other circumſtances produce ſimilar effects. In general the ſenſe of cold predominates, even after a morbid heat has taken place at the axilla, under the tongue, and in different parts of the abdomen and thorax; becauſe a conſiderable part of the ſurface, and the whole of the extremities, ſtill remain below the natural temperature, and the ſenſation which this produces, ariſing from parts of great ſenſibility, overcomes the ſenſation of heat from the deeper ſeated parts, whoſe ſenſibility is ſo much inferior. At length however the heat of the ſurface becomes general and uniform, riſing to 102°, 103°, 104°, and ſometimes 105° of Fahrenheit. I have not ſeen it higher than this laſt degree in the paroxyſm of intermittent, and ſeldom in continued fever, though different authors ſpeak of febrile heat four, or even five degrees higher. The ſenſation of heat is now ſtrong and ſteady, and the acceſſion of external air does not produce a return of chillneſs as before.—This is the time for the affuſion of cold water, as has already been mentioned. The ſenſation of heat is moſt powerful on the extremities, particularly on the palms of the hands and the ſoles of the feet.—This ariſes in part from the great ſenſibility of theſe parts, but in part alſo from this circumſtance, that in their natural ſtate they are two or three degrees cooler than the trunk of the body, and cannot be raiſed to the general temperature of health without an uneaſy ſenſe of heat, which is frequently relieved by a partial perſpiration. When raiſed to the general fever-heat, their temperature is therefore increaſed in a more than a proportional degree beyond its uſual ſtandard.

After remaining ſome time in this hot ſtage, the ſkin begins to relax, and to become ſofter and ſmoother to the touch ; inſenſibly a moiſture takes place, terminating generally in profuſe perſpiration. Neither however does this proceſs go on regularly ; after the moiſture commences, it ſometimes goes off, and a dry and burning ſtate of the ſkin returns : again the ſweat recommences, and when it becomes general and continues ſteady, it is critical, that is, it terminates the paroxyſm. As it commences, the heat declines, though at firſt very ſlowly : as it proceeds, the heat ſubſides gradually ; and when it terminates, the temperature of health is reſtored. The ſinking of the morbid heat is however a good deal regulated by the quantity of the bed-clothes, and the cloſeneſs with which the body is enveloped.—Where the whole covering that was laid on during the cold ſtage, is kept on during the whole paroxyſm (as often happens through a miſtaken wiſh to force the perſpiration) the heat is carried off with difficulty, and it requires a very profuſe perſpiration to reduce it to its natural ſtandard. On the contrary, if the clothes be lightened after the hot ſtage is fairly eſtabliſhed, the heat paſſes off with leſs difficulty, and the perſpiration requiſite to remove it is much leſs profuſe.

Theſe details will not be thought ſuperfluous or tedious by thoſe who conſider, that after all that has been written on the ſubject of fever, thermometrical obſervations on the riſe and progreſs of febrile heat, are in a great meaſure new*.

5. Whether it be owing to the nature or force of the contagion; to the ſtate of the conſtitution; or to ſome other unknown cauſe; the re-action of the ſyſtem does not always reſolve the ſpaſm or morbid ſtricture on the extreme veſſels. When this ſtricture is not reſolved, the ſweat is not effuſed, the morbid heat is not carried off, and the diſeaſed actions run into continued fever.

If a perſon is confined in a hot room, or in the hot bath, till his heat riſes four or five degrees above the natural ſtandard, his pulſe will be found of a feveriſh rapidity; wandering pains will ſoon be felt over the body; languor, laſſitude, and at length great debility will take place, with moſt of the ſymptoms of regular fever. It is evident that theſe ſymptoms cannot be expected to go off till the inordinate heat is removed, and if the perſon remain ſome time in the heated medium, he will find that the inordinate action of the heart and arteries continues after leaving it, and even after his own heat has ſubſided to its natural ſtandard. This depends on a principle peculiar to life, to which the name of habit or aſſociation has been given, and which extends its influence to all the vital phenomena, whether intellectual or corporeal.

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* I intended to have introduced here one or two regiſters of the heat and pulſe, taken every half hour during the paroxyſm of intermittent, but this is delayed till I am enabled to ſpeak from more numerous obſervations.

In fever this morbid heat does not ariſe from the ſurrounding medium, but from certain motions in the ſyſtem itſelf. But however generated, a heat five or ſix degrees above the natural ſtandard muſt be a powerful agent, and it cannot be expected that the diſeaſed actions ſhould ſubſide under ſo ſtrong a ſtimulus. The rapidity of the circulation, and the labour of reſpiration, are conſequences of a heat of this degree, from whatever cauſe ariſing, and muſt continue till the heat is reduced.

We may here obſerve, that it is not by any means eaſy to make an accurate compariſon between the ſtate of the ſyſtem under the heat of fever, and under an equal degree of heat introduced from the ſurrounding medium. Becauſe it is in fact very difficult to raiſe the heat of the body in a ſtate of health, to the higheſt degree of fever heat, either by heated air, or heated water. The experiments recorded by Sir Charles Blagden, prove this in reſpect to a heated atmoſphere; the heat as it is accumulated in the ſyſtem, being diſſipated by profuſe perſpiration, or by ſome other living proceſs, of which perſpiration is a concomitant effect. I have found the ſame difficulty in heating the body in the hot bath, for there alſo the heat is with difficulty increaſed beyond 100° or 101°, when ſweat, as is uſual, breaks out over the ſurface. Even in theſe degrees the greater part of of the ſymptoms of fever will be felt. The heat however is moderated, and, when the temperature of the external medium is reduced, it is carried off by the diſcharge from the ſurface. But in fever this is prevented; for the peculiar debility induced by the remote cauſe, occaſions, or is attended by, a ſpaſm, or morbid ſtricture, of the capillaries of the ſurface and of the ſkin itſelf, by which the inſenſible perſpiration is leſſened, and the ſenſible perſpiration obſtructed. Hence the difficulty of reducing the heat in fever; and thus it appears, that the ſpaſm on the extreme veſſels, which ſome of our lateſt phyſiologiſts have wholly neglected, is perhaps the moſt important peculiarity of the diſeaſe.

It may be obſerved, and it furniſhes a ſubject of intereſting inquiry, that this ſtricture, which often gives way under a moderate re-action, maintains itſelf with rigidity where the re-action is great. Thus where the heat riſes to 99°, 100°, or 101°, the orifices of the ſkin often relax, and carry it off by increaſed perſpiration, while at 104° and 105°, with a proportional force of circulation, they are in general obſtinately conſtricted. This is very ſtriking in fever, but not peculiar to it. Even in the ſtate of health, while a moderate, gradual, and general ſtimulus from heat opens the pores of the ſkin, a ſudden and conſiderable increaſe of heat conſtricts them; the violence of the ſtimulus, in this, as in other inſtances, occaſioning a morbid reſiſtance of the living fibre on which it acts. This reſiſtance to inordinate ſtimuli extends to every part of the living ſyſtem, and belongs to every ſpecies of vital action; it characterizes the mental as well as the corporeal functions, and a full conſideration of its influence might afford the moſt important inſtruction, not to phyſicians only, but to moraliſts and legiſlators!

But, it may be juſtly remarked, that after fever is eſtabliſhed, the reduction of heat to the natural ſtandard, however deſirable, is attended with difficulty and hazard, ſince the patient cannot in general bear the continued expoſure to external cold, neceſſary for this purpoſe.

Even in the hot ſtage of fever, if the patient be expoſed naked to a cold atmoſphere, though he ſupports the application for ſome time with comfort and even with advantage, yet as the heat of the ſurface approaches the ſtandard of health, a ſudden ſenſe of chillneſs comes on, with a return of oppreſſion on the heart and lungs, and all the ſymptoms of the firſt ſtage of the paroxyſm. The length of application, and the degree of external cold producing this effect, are indeed various in various conſtitutions, and in different ſtages of the diſeaſe: the degree of reduction of the animal heat at which the chillneſs commences, is alſo uncertain; but the general truth of the poſition laid down is eſtabliſhed on ample experience, and will not, I believe, be controverted. In cooling the ſurrounding atmoſphere in fever, or in any other continued application of cold, care muſt be taken to keep it within the limits in which it is grateful to the ſenſations; and within theſe limits the cool treatment, as far as my obſervation extends, is uniformly advantageous. But though it moderates re-action, it does not remove the ſpaſm on the capillaries, or break the catenation by which fever is prolonged.

This is effected by the ſudden affuſion of cold water over the naked body; and the mode of its operation will now be eaſily underſtood. The ſudden, general, and powerful ſtimulus given to the ſyſtem, diſſolves the ſpaſm on the extreme veſſels of the ſurface, and of the various cavities of the body: the ſudden and general evaporation carries off a large portion of the morbid heat accumulated under the ſkin; and the healthy action of the capillaries and exhalents being reſtored, the remaining ſuperfluous heat paſſes off by ſenſible and inſenſible perſpiration. The ſtimulus of morbid heat and of morbid ſtricture being removed, the inordinate action of the heart and arteries ſubſides, and the haraſſed, and toil-worn patient ſinks into that peaceful ſleep which nature has provided as the ſolace of our pains and ſorrows, and the reſtorer of our ſtrength.

If ſuch be the explanation of the diminution of the heat, and of the frequency of the pulſe; which follow the affuſion of cold water on the ſurface, it may here be inquired how the diminution of the thirſt is accounted for. Are we to ſuppoſe, that a portion of the liquid is taken up by the abſorbents of the ſurface, as is generally believed to be the caſe, during immerſion in the bath? I apprehend not. The abſorption in the bath, if it occur at all, (of which there is no ſufficient evidence) has been greatly exaggerated, and the diminution of thirſt after the cold affuſion, is too ſpeedy to be aſcribed to ſuch a cauſe. It admits of a more ſatisfactory explanation. The morbid ſtricture on the extreme veſſels, to which we impute ſo much, is not confined to the capillaries of the ſurface, but extends, as far as we have an opportunity of obſerving, to the capillaries of all the cavities of the body. The ſenſe of thirſt is produced by the heat and dryneſs ariſing from the morbid ſtricture of the exhalents in the inner ſurface of the mouth, fauces, and ſtomach; and this ſtricture being removed by the affuſion on the ſurface of the body, from the ſympathy between the ſurface and theſe cavities, ſo often alluded to, the healthy action of the exhalents of theſe parts, as well as thoſe of the ſkin, is reſtored, and pouring out their moiſture, the heat and dryneſs are removed, and the thirſt diminiſhed.

This indeed ſeems to be the way in which thirſt is relieved in the firſt inſtance, by liquids taken into the ſtomach itſelf, where the effect is too ſudden to ariſe from the abſorption of any portion of the liquid into the maſs of circulating fluids. We cannot indeed perceive this effect on the exhalents of the ſtomach, but we may reaſonably ſuppoſe it to take place where we obſerve the ſwallowing of drink to be followed by a relaxation of the exhalents on the ſkin, which muſt proceed from their ſympathy with thoſe of the ſtomach previouſly relaxed; and it may illuſtrate and ſupport this poſition, that when liquids ſwallowed even in large quantities in the hot ſtage, do not produce a ſenſible increaſe of perſpiration on the ſkin, the relief they afford to the thirſt is momentary only. Doubtleſs in ſuch caſes the ſtricture on the exhalents of the ſtomach has not been reſolved. The relief from thirſt afforded by liquids taken into the ſtomach is in general however more effectual and complete, than from affuſion of water on the ſurface, becauſe when the ſtricture on the capillaries is removed, a rapid abſorption takes place from the ſtomach, and the circulating fluids are diluted. But on the ſurface of the body this abſorption is doubtleſs ſlow and imperfect, and it will be afterwards ſhown, that there is reaſon to doubt whether it takes place at all. It was remarked that the affuſion of tepid water on the ſurface, though effectual in diminiſhing heat, (page 68) is not followed by the entire ceſſation of fever, as is often the caſe after the cold affuſion. The cauſe of this is obvious—the coolneſs produced by the tepid affuſion ariſes from the increaſed evaporation, and continues only while this continues. The ſtimulus given by the tepid affuſion is comparatively ſlight and tranſient—it does not reſolve the ſpaſm on the ſurface, or on the other cavities of the body, and of courſe it does not deſtroy the morbid catenation. The heat therefore ſpeedily returns, when the remedy is withdrawn.

This explanation of the operation of the affuſion of cold water ſeems to me ſatisfactory. The caſes which have been related ſhow, however, that it is only in the early ſtages of fever that it produces a ſolution of the diſeaſe. In the after ſtages, though uniformly advantageous while the morbid heat continues,

tinues, its effects are not ſo deciſive, the morbid actions having acquired the force of habit, or in other words being carried on by the aſſociations or catenations that have been produced.

What then ought to be the indications of practice in fever? To diminiſh the cold in the cold ſtage; to moderate the heat in the hot ſtage; and to reſolve the ſtricture on the extreme veſſels by which the heat is retained, and the re-action prolonged; and where the inordinate action of the vaſcular ſyſtem continues after theſe objects have been attained, to ſupport the powers of life till the morbid aſſociations, or habits of action, gradually die away, from the removal of the cauſes by which they were introduced. In addition to theſe general indications, it will be eſſential to ſecure the proper action of the bowels; and in every caſe to unload the alimentary canal of its morbid contents; whether theſe contents have become diſeaſed through the action of general fever, or, as there is reaſon to believe in ſome of the fevers of the warm climates, be the remote cauſe by which fever is produced.

If theſe general obſervations ſhould excite attention, they will doubtleſs excite objections alſo, and perhaps animadverſions; but whatever be the fate of his reaſonings, the author reſts with ſome confidence on the ſtability of the facts by which they have been ſuggeſted. In the view which he has taken of fever, the ſuppoſed intentions of nature are not inquired into; and of courſe no attempt is made to arrange the ſymptoms as they ariſe from the direct agency of the febrile poiſon, or the exertion of *her* medicating power. Such attempts are as unneceſſary as they are difficult. It is not indeed to be denied, that the living ſyſtem, after being excited into morbid action, paſſes frequently, unaſſiſted, through a ſucceſſion of ſymptoms into a ſtate of health; differing in this as in every other reſpect, from any of the arrangements of unorganized matter. But it is equally true that it often ſinks in the courſe of this proceſs, ſometimes deſtroyed in fever by the exceſs of its own re-action, and ſometimes by the habits of action, or aſſociations produced in the courſe of the diſeaſe.

It is a ſerious error to ſuppoſe that the febrile poiſon, being received into the ſyſtem, is the principal cauſe of the ſymptoms, and that they conſiſt of a ſtruggle of nature to expel it, without which health cannot be reſtored. It is ſafer to conſider the febrile poiſon as an agent that excites the ſyſtem into fever, which however is carried on, not by the continued preſence and agency of this poiſon, but by the principles which regulate the actions of life. We are not therefore to wait for the ſanative proceſs, by which nature is ſuppoſed to ſeparate this *virus*, and to throw it off, watching her motions, and aſſiſting her purpoſes; but to oppoſe the fever in every ſtage of its progreſs with all our ſkill, and to bring it to as ſpeedy a termination as is in our power. When we diſpel the morbid heat, and reduce morbid re-action in the hot ſtage of the original paroxyſm, by the powerful means of the cold affuſion, the whole of the febrile ſymptoms vaniſh; a ſufficient proof that in this ſtage of the diſeaſe theſe ſymptoms aroſe from inordinate heat and inordinate action, and not from a poiſon circulating with the blood. Hence the ſafety and the wiſdom of deciſive meaſures in the earlier ſtages, before the ſtrength is materially impaired, or the diſeaſed habits eſtabliſhed. Thoſe who practice within the tropics, where fever runs its dreadful courſe with ſuch rapidity, ought eſpecially to be aware of this truth, with which indeed the moſt enlightened of their number ſeem to be ſufficiently impreſſed. The writings of Dr. Jackſon and Dr. M^c^Lean inform us, that the affuſion of cold water begins now to be employed in the deſtructive diſeaſe (popularly named the yellow fever) of Jamaica and St. Domingo, and with ſalutary effects*. In thoſe climates, however, the diſeaſe ought to be combated, not merely by affuſion of cold water on the ſurface, but by immerſion of the patient in the cold bath. In this fever, hours, nay minutes are precious, and as the cold ſtage is ſhort, the application of this remedy admits little delay. As ſoon as the ſecond, or hot ſtage is formed, a judicious and reſolute application of cold might ſuperſede all other remedies. But the temperature of the ſea, and even of the ſprings within the tropics, riſing generally as high as 77° or 78° of Fahrenheit, affords a ſtimulus to the ſenſations from ſimple affuſion, of a weaker kind, and hardly permits us to hope, that in the form of affuſion the cold bath can be followed by thoſe effects in the ardent fever of the torrid zone, which in the milder fever of this climate it ſo happily produces. Artificial modes of cooling the water may indeed be reſorted to, and when this can be effected, the repeated

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* See Dr. Jackſon on the "Fevers of Jamaica." *London, printed by Murray*, 1791, and Dr. McLean's inquiry into the nature and cauſes of the great mortality among the troops in

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St. Domingo.—*London*; *Cadell and Davies*, 1797. Had I ſeen Dr. Jackſon's work earlier, I ſhould have mentioned him ſooner; but I was referred to him for the firſt time by the work of Dr. McLean, juſt publiſhed, and which reached me after this chapter was nearly printed off. The works of theſe experienced phyſicians deſerve great praiſe; but their accounts of the fevers of the Weſt Indies—like all the other accounts, are without information in regard to the actual ſtate of the heat of the patients. The thermometer has not yet been employed in the diſeaſes of the tropics.

repeated uſe of the affuſion may in all probability extinguiſh the diſeaſe. But on military duty, as well as in many other ſituations, artificial methods of cooling a ſufficient quantity of water it may often be impoſſible to employ. In ſuch caſes, when the the phyſician has aſcertained that the heat of his patient has riſen to the degree which juſtifies the practice, let him plunge him fearleſsly into the cold bath, or the ſea. The duration of immerſion muſt depend on the effects on the pulſe, on the ſenſations, and on the heat meaſured by the thermometer; and a greater degree of coolneſs will be produced by alternately raiſing him into the air, where the wind blows over his naked body, and ſinking him in the water, than by continued immerſion. *(See Appendix, No.* II.) The utmoſt care is neceſſary in a proceſs of this kind, to guard againſt the effects of fatigue. Various inſtances might be adduced of the ſucceſs of this practice in perſons who have in the delirium, of fever on ſhipboard, plunged into the ſea—The caſe of Sir John Chardin, who, when at Gambroon in the year 1673, was cured by this means of a burning fever, attended by delirium, after every other remedy had proved ineffectual, is a celebrated inſtance of this kind, and long ago ſuggeſted to Dr. Glaſs the propriety "of making a farther trial of this uncommon and ſurpriſing "remedy." I have received a variety of authentic relations of the ſame nature. Theſe have happened on board our ſhips at ſea, during the delirium of fever, and chiefly in the warm climates; and in every inſtance where the patient has been fortunately taken up, recovery has followed.

An accident of this kind occurred about three years ago to Captain S——, of this port, in the Iriſh channel. He ſprung out of his cabin window in the height of delirium, and was upwards of twenty minutes in the water. He was taken up perfectly calm, and ſpeedily recovered.—If it be conſidered, that in the Iriſh channel, even in ſummer, the temperature of the water is ſeventeen or eighteen degrees colder than in the Weſt Indies, the importance of this fact will be clearly perceived. But beſides the different modes of applying cold water to the ſurface, it ought to be poured into the ſtomach in large quantities, when the patient's heat will permit it, and the preſence of nauſea and vomiting is no objection to this practice, if a chillneſs of the ſtomach is not produced.

Conſidering the general adoption of cold drink in the ardent fever of the ancients, its ſucceſs in the Hungarian fever, to which the yellow fever bears ſuch analogy, and the pointed recommendation it has received from Hoffman in bilious vomitings and dyſentery, it appears rather extraordinary that it has not hitherto been reſorted to in the diſeaſes of the Weſt Indies.

The ſalutary effects of the cold bath, and of cold drink in fever, ſtrongly recommend the adoption of theſe remedies in the plague. On the coaſts of the Mediterranean, where the liberal uſe of cold drink in fevers prevailed throughout antiquity, and on ſome parts of which it ſeems to have been carried to a great extent in the beginning of the preſent century, there is reaſon to believe that it has now fallen into diſuſe. If we give credit to the anſwers made to the queries of Mr. Howard, in regard to the treatment of the plague, the adminiſtration of cold drink forms no part of the regular practice in that terrible diſeaſe. The Jew phyſician of Smyrna indeed ſays, "that the Turks in the "violence of the fever, take handsfull of ſnow and "apply it over their bodies, and alſo eat it; and "likewiſe ſometimes throw cold water on their "feet." But the learned Hebrew cannot determine whether it is of ſervice, "as theſe people in "other reſpects pay no regard to the rules of diet." *Howard on Lazarettos, page* 39. Morandi, phyſician at Venice, obſerves, "that ſome ſailors at Con"ſtantinople, in the phrenzy of the plague, have "thrown themſelves into the ſea; and it is ſaid, "that on being taken out, they have recovered." But this happy temerity the regular practitioners do not appear to have imitated, a fact that is further eſtabliſhed by Dr. Ruſſel, in his account of the treatment of the plague at Aleppo.

The adminiſtration of cold drink in the plague, as well as the external application, muſt however be regulated by the actual ſtate of the patient's heat, and of his ſenſations of heat, of which alſo we have to lament that we have no accurate information. If it ſhould be found that in this malignant diſeaſe the heat is little or not at all above the natural ſtandard, (as is the caſe in the advanced ſtages of the confluent ſmall-pox) our hopes from the exhibition of this powerful remedy would probably be diſappointed. But the trial is recommended by every conſideration*.

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* When Mr. Howard was printing his work on *Lazarettos*, at Warrington in 1788, I communicated to him an account of the ſucceſſes of our firſt trials of the affuſion of cold water in fever, in the Liverpool Infirmary, and he promiſed to make more particular inquiries in regard to the empirical uſe of this remedy, in his enſuing journey to the Eaſt, in the earlier part of which, it is well known, his career of benevolence terminated.

CHAP. XV.

Animal heat—its origin.—Perſpiration—its cooling influence.—Mode of operation of other remedies in Fever—Antimonials—Opium, &c.

I HAVE elſewhere obſerved, that if a definition of life were required, it might be moſt clearly eſtabliſhed on that capacity, by which the animal preſerves its proper heat under the various degrees of temperature of the medium in which it lives. The more perfect animals poſſeſs this power in a ſuperior degree, and to the exerciſe of their vital functions this is neceſſary. The inferior animals have it in a lower degree, in a degree however ſuited to their functions. In vegetables it ſeems to exiſt, but in a degree ſtill lower, according to their more limited powers, and humbler organization. As the capacity of preſerving nearly an uniform temperature in all the varieties of climate and ſeaſon, is the criterion of life in the more perfect animals, ſo among individuals of the ſame ſpecies, the degree of this capacity may be conſidered as the criterion of the ſtrength of the living principle.

There is reaſon to believe, that while the actual temperature of the human body remains unchanged, its health is not permanently interrupted by the variation in the temperature of the medium that ſurrounds it; but that a few degrees of increaſe or diminution of the heat of the ſyſtem, produces diſeaſes and death. A knowledge therefore of the laws that regulate the vital heat, ſeems to be the moſt important branch of phyſiology.

Modern chemiſtry aſſumes to itſelf the diſcovery of the origin of animal heat, which is ſuppoſed to depend on thoſe changes in the ingeſta, which the functions of life are always producing. Among the breathing animals, the principal of theſe changes is occaſioned by the lungs on the atmoſpheric air which they inhale, and reſpiration is conſidered as the principal ſource of the heat of this claſs of animals. That the oxygen contained in the atmoſphere furniſhes the largeſt portion of the pabulum of life, is no longer doubted, reſpiration being a proceſs in which it paſſes from its gaſeous to its concrete ſtate, giving out a portion of the heat it held in vapour; in the ſame manner as ſteam, in its converſion into water, gives out a portion of its heat. The heat thus extricated, being conveyed by the circulation to all the parts of the body, is the principal means by which the whole is warmed and animated.*

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* Reſpiration is a caſe of compound elective attraction, in which the oxygen gas is decompoſed, and enters into new

Some phyſiologiſts of our own country, and on the continent, have of late ſuppoſed, that a ſimilar proceſs is carried on by the ſurface of the body, through which alſo, though in an inferior degree, the oxygen of the atmoſphere is inhaled. Experiments are wanting to eſtabliſh this poſition, or to ſhew us how far it extends, and there is reaſon to believe that the principal function of the ſkin, in the breathing animals at leaſt, is of a different and oppoſite nature. That an animal poſſeſſes to a certain extent, the faculty of rendering ſenſible heat latent, or to ſpeak more philoſophically, of reſolving heat into caloric, in caſes in which the ſtimulus of heat might otherwiſe overpower the living energy,

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compounds; a part unites with the iron of the blood, and forms it into an oxyd; a part with the hydrogen of the venous blood, and forms water; another part with the carbon of this blood, and forms carbonic acid gas. In the two firſt caſes the language in the text applies ſtrictly, but in the laſt caſe the heat extricated does not ariſe from the oxygen becoming concrete, but from its entering into the compoſition of a gas which has a leſs capacity for caloric than itſelf. The heat diſengaged in theſe different proceſſes, unites with the blood, which in loſing its hydrogen and carbon (hydro-carcarbonat) is converted from venous to arterial, and has its capacity for caloric increaſed. Thus inflammation in the lungs is prevented. But the arterial blood, in the courſe of circulation, again abſorbs by little and little a portion of hydrocarbonat, parting with caloric in the ſame proportion. Hence the uniformity of temperature throughout the body. This ſeems the modern theory of reſpiration, which it muſt be confeſſed is not without its difficulties.

energy, there is reaſon to believe, from a variety of experiments and obſervations. And that this is in part performed by the perſpiration from the ſurface, can ſcarcely admit of a doubt. The proceſs of perſpiration which is continually going on from every part of the body, is in this point of view the converſe of that of reſpiration: as in reſpiration a gas is conſtantly converted into a ſolid or fluid, and thus heat evolved, ſo in perſpiration a fluid is continually converted into a vapour, and thus heat is abſorbed. If then we ſuppoſe, that while the proportion of oxygen received into the ſyſtem continues the ſame, the temperature of the atmoſphere is increaſed, we can underſtand why our heat is not increaſed, by ſuppoſing an increaſe of perſpiration. And if the temperature of the atmoſphere remains unchanged, while the oxygen received by the lungs is increaſed, we can ſtill explain the ſtability of our heat, by ſuppoſing an increaſe of perſpiration. The firſt of theſe ſuppoſitions is nearly realized, when a warm day comes on after cold weather; the ſecond is realized when an increaſed reſpiration takes place under exerciſe—thus perſpiration appears to have a principal ſhare in regulating the animal heat, and the chain of life ſeems connected with the phyſical world by two links, which the recent diſcoveries in chemiſtry enable us to unveil*.

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* I have ſaid *nearly* realized, in the beginning of the ſentence. Suppoſing the ſame bulk of air breathed in warm as

A veſſel filled with water, and expoſed to the air, cannot be raiſed above 212° of Faht. by any quantity of fuel, becauſe, as heat is applied from below, evaporation carries it off from the ſurface. Hot ſprings are of the ſame heat at their fountains throughout the year, probably becauſe the evapora-

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in cold weather, the proportion of oxygen will be rather leſs in the firſt caſe, from the diminiſhed denſity of the air. There is reaſon to believe alſo, that an animal contaminates the air more ſlowly in warm weather, that is, receives a leſs proportion of the oxygen it inhales, into the ſyſtem. But theſe particulars, in the rapid and general views which I offer, are neceſſarily overlooked.

Dr. Mitchel, of New York, in his ingenious paper on the nature of contagion, has unguardedly aſſerted, that the actual heat of the animal is always proportioned to the quantity of oxygen inhaled; but a very curſory examination of facts will ſhew that this is erroneous. By the experiments in No. II. of the Appendix (and various others might be adduced) it will be ſeen, that the living body has the power of preſerving its heat nearly unchanged for a conſiderable length of time in ſo denſe a medium as water 50° colder than the blood. This could not ariſe from an increaſed inhalation of oxygen. The phenomenon is partly to be explained from the ſudden contraction of the perſpiratory organs; but principally, from the powerful impreſſion on the ſenſations rouſing the vital principle to increaſed exertion, and to increaſed extrication of heat. It is to be obſerved too, that it was the trunk, and more ſolid parts of the body, that preſerved their heat, in the experiments referred to; the extremities became cold.

tion from the ſurface of their waters increaſes and diminiſhes with the heat of the atmoſphere. In like manner we may ſuppoſe the heat of the living body to be kept uniform by the evaporation from its ſurface increaſing or diminiſhing according to the quantity of heat extricated in the ſyſtem, or received from the ſurrounding medium. But the cautious reaſoner, aware of the deceptions ariſing from ſuch analogies, will here very properly inquire—Does the perſpiration by the ſkin, the body remaining at the temperature of health, actually go on more rapidly in warm than in cold weather? Is it greater under exerciſe than when at reſt? Is it more plentiful when oxygen is received in abundance into the ſyſtem, than when it is imbibed more ſparingly? We run little hazard in anſwering the two firſt of theſe queſtions in the affirmative, but in regard to the laſt, farther experiments are requiſite to enable us to decide.

The inſenſible perſpiration attracted the notice of phyſicians very early, and it was a common dogma among the ancients, which has deſcended with too little examination to our own times, that the whole ſurface of the body inhales and exhales. The ſubject has been conſidered more accurately ſince the experiments of Sanctorius in the middle of the laſt century, and the great quantity of matter ſuppoſed by him to paſs off by this proceſs, pointing out its importance in the animal œconomy, theories of diſeaſe, founded on the diminution or obſtruction of this diſcharge, have ſince his time prevailed univerſally. The quantity of the matter of perſpiration is not eaſily aſcertained, and this uncertainty muſt continue while the abſorption by the lungs, (and perhaps by the ſkin,) from the atmoſphere, remains uncertain. It was for ſome time believed, that the quantity of perſpiration from the ſurface, had been exaggerated by Sanctorius, but ſince a notion has been entertained of the great abſorption by the ſkin, his calculations have been ſuppoſed to fall ſhort of the truth. If however we calculate that fifty ounces of water are converted into vapour on the ſurface of the body in twenty-four hours, this muſt produce a very conſiderable abſorption of heat, and have a powerful influence on the temperature of the living ſyſtem.*

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* Very various calculations have been made of the quantity of matter perſpired. The only experiments on this point that deſerve much notice, are thoſe of Mr. Cruikſhanks. He appears to have loſt upwards of ſeven pounds weight in this way in twenty four hours, when at reſt, in an atmoſphere of 71°.; but when exerciſing, a much larger quantity. This exceeds the calculation of Sanctorius, and the experiments of Mr. C. point out what Sanctorius did not know, the great abſorption by the lungs; for there is reaſon to believe, that the abſorption of air, vapour, or water, by the ſkin, if it occur at all, has been much exaggerated.

And however we may diſtruſt the experiments hitherto made, on the quantity of the matter of perſpiration, there is little reaſon to queſtion the points in which they all agree—that this diſcharge is greater in warm weather than in cold—under exerciſe, than when at reſt.

Important as the evaporation from the ſurface of the body muſt be in regulating the heat, it is remarkable how little it has hitherto been conſidered in this point of view. In the year 1755, Dr. Cullen publiſhed his diſcovery of the cold produced by the evaporation of fluids, (*Eſſays and Obſervations Phyſical and Literary, vol.* ii.) a phenomenon long known in Aſia, but till then unobſerved in Europe, and which has paved the way to ſo many other diſcoveries of the modern chemiſtry. It does not however appear that Dr. Cullen applied his diſcovery in explaining the function of perſpiration. The effects of evaporation from the ſurface in cooling the body, was however ſoon after noticed by Dr. Franklin. In a letter, dated *June* 1758, is the following obſervation, " During the hot Sunday at
" Philadelphia, in June 1750, when the thermome-
" ter was up at 100° in the ſhade, I ſat in my cham-
" ber without exerciſe, only reading or writing, with
" no other cloaths on than a ſhirt and a pair of long
" linen drawers, the windows all open, and a briſk
" wind blowing through the houſe; the ſweat ran
" off

" off the backs of my hands, and my ſhirt was often " ſo wet as to induce me to call for dry ones to put on. " My body however never grew ſo hot as the air, or " the inanimate bodies immerſed in the air.*" It does not indeed appear that Dr. Franklin actually meaſured his heat, but he makes the inference of his comparitive coolneſs from remembering, that all the bodies about him, even the ſhirt out of his drawer, felt warm to the touch, and he concludes that he was kept cool " by the continual ſweating, " and by the evaporation of that ſweat." Doubtleſs this reaſoning was ſuggeſted to Dr. Franklin, and perhaps the whole circumſtances recalled to his mind, by the paper of Dr. Cullen, then recently publiſhed. When, by the obſervations of Meſſ. Du Hamel and Tellet, in France, and the experiments of Dr. Fordyce and Sir Charles Blagden in England, a diſcovery was made of the very extraordinary degrees of heat which the living body can ſupport, philoſophical minds naturally endeavoured to account for ſo ſingular a phenomenon. That the animal poſſeſſes a power of generating cold as well as heat, was the more general opinion; but the concluſiveneſs of the experiments of our Engliſh philoſophers to eſtabliſh this opinion, was queſtioned by many, and among others by my late ingenious friend Dr. Bell, in the paper that forms the firſt number in the *Memoirs of the Society of Mancheſter.* In this paper the effects of the evaporation from the ſurface in reducing the heat during the experiments in queſtion, is particularly inſiſted on. In diſcuſſing this ſubject with Dr. Bell, it naturally ſuggeſted itſelf, that the principal office of the inſenſible perſpiration might be to regulate the animal heat, and this opinion, which ſeems ſo reaſonable, has been confirmed by reflection and obſervation.

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* *See Franklin's Letters and Papers, p.* 365.

But whatever may be the influence of the inſenſible perſpiration in cooling the body, it does not ſeem to be the only proceſs by which this effect is produced. From ſome experiments in the hot bath, it appears to me that the temperature of the body is with difficulty increaſed after the ſweat begins to flow profuſely, and as there can be no evaporation from the ſkin when the body is immerſed in water, ſhould my obſervation be confirmed by future experience, it will ſhew, that in the production of ſweat itſelf, a degree of heat is abſorbed, and thus explain in ſome degree the reduction of heat that follows profuſe perſpiration in other caſes. It was indeed ſuppoſed by Albinus, Haller, and William Hunter, that the ſweat, as well as the inſenſible perſpiration, is an exudation of the watery part of the blood through the cuticle; but this opinion is contrary to all analogy, and depending on experiments made on the dead body, may be ſafely rejected, notwithſtanding the illuſtrious names that countenance it. The opinion of Dr. Fordyce and Mr. Cruickſhanks ſeems to be the true one. The matter of perſpiration is ſeparated from the blood by the capillary arteries, and thrown out on the ſurface by organic pores in the cuticle (however difficult to be diſcovered) connected with the extremities of theſe arteries; and in this proceſs there is not a ſeparation merely, but a new combination, as in ſimilar inſtances. In this combination there may be an abſorption of heat, and thus the coolneſs produced by ſweating be in part accounted for, The abſorption of heat may however take place in various other proceſſes, beſides the production and evaporation of perſpirable matter; as doubtleſs its extrication takes place in various other proceſſes beſides reſpiration; and after all, the ſudden changes of temperature that take place in certain circumſtances, render it probable that the animal poſſeſſes powers over its heat not yet underſtood, and which no chemical theories can explain.

Leaving this point for future inquirers, the importance of perſpiration to the animal œconomy may ſafely be affirmed. By this proceſs the ſtimulus on the extreme veſſels ariſing from morbid diſtenſion, is diminiſhed or removed, and whatever other methods the ſyſtem may poſſeſs of diminiſhing its heat, this diſcharge ſeems in certain circumſtances eſſential to that object. In ſituations where the organs of perſpiration are ſpaſmodically conſtricted, while by the increaſed momentum of the circulation, heat is præternaturally evolved (as in the commencement of fever) we can eaſily underſtand the diſorder of the ſyſtem that enſues.

It is obſerved, that perſons who engage in exceſſive hard labour, ſpeedily ſink under it, unleſs they perſpire freely, and ſupport their perſpiration by drinking ſome thin and moderately ſtimulating fluid. This is the caſe with the reapers in Pennſylvania, where the harveſt occurs in the hotteſt ſeaſon of the year, and who by means of profuſe perſpiration, are enabled to work in the ſunſhine, in which the thermometer riſes very often above the heat of the blood.* This is the caſe with our coal-heavers, who probably loſe a fifth or ſixth part of their whole weight by this diſcharge daily, and who ſupply this waſte chiefly by large draughts of porter. In inſtances of ſuch extreme labour, a ſudden ſtoppage of perſpiration from the ceſſation of exertion, and the acceſſion of cold, is frequently fatal, as has already been explained.

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* *See Franklin's Letters, and Papers, p.* 366.

Europeans who go to the Weſt Indies are more healthy in proportion as they perſpire freely, eſpecially if they ſupport the diſcharge by a moderate uſe of gently ſtimulating liquids, ſtopping ſhort of intoxication; and guard againſt the effects of too exceſſive an evaporation when the vigour is impaired by fatigue. In the extreme heats of the torrid zone, this indeed in not eaſy. The highly excitable ſyſtem of the youthful European, is acted on powerfully by the climate. If the orifices of the ſkin do not pour out a proportional quantity of perſpiration, diſeaſe muſt enſue from the direct ſtimulus of heat; and if the neceſſary quantity of perſpiration takes place, the ſyſtem is enfeebled by the evacuation, and the extreme veſſels loſing their tone, continue to tranſmit the perſpirable matter after the heat is reduced as low, or perhaps lower than its natural ſtandard—as occurs in the cold colliquative ſweats in the latter ſtages of fever. In this ſituation we can eaſily ſuppoſe, that even a ſlight degree of external cold may become dangerous. The damps and chillneſs of night, joined to the increaſed evaporation produced by the night-breeze, muſt often prove pernicious, eſpecially after exerciſe and profuſe perſpiration. They rob the ſurface of its neceſſary heat, produce a torpor and contraction of the extreme veſſels, and the dangerous re-action of the centre, which has already been explained. The proper treatment of the European on his landing in the Weſt Indies, may probably conſiſt in his being placed for a time in ſome inland and elevated ſtation, where the air is leſs ſultry than on the coaſt; on his avoiding as much as poſſible any violent exerciſe; on his guarding againſt all intoxicating liquids, and whatever increaſes the heat and diminiſhes the ſtrength of the body; taking care however to ſupply the circulation with liquids proportional to the waſte. By ſuch means his ſyſtem will gradually accuſtom itſelf to the climate, the organs of perſpiration adapt themſelves to the increaſed heat, and the dangers of ſudden change be averted.

The perſpirable matter of the European, does not appear to be well fitted to the torrid zone. Conſiſting nearly of pure lymph, it is ſpeedily diſſipated by evaporation, and without a profuſe diſcharge, the ſurface cannot be kept moiſt and cool. On the other hand, the profuſion of the diſcharge debilitates the ſyſtem, and renders even the cold of evaporation dangerous, eſpecially when this evaporation is increaſed by an accidental breeze, or the effect of the cold promoted by reſt after fatigue. The negro, fitted by nature to the climate, is leſs acceſſible to the ſtimulus of heat, and *his* unctuous ſweat leſs eaſily diſſipated, keeping his ſkin uniformly moiſt, ſuſtains a more uniform perſpiration as well as evaporation, and guards the ſyſtem againſt the waſte and danger of profuſe ſweating under fatigue, as well as the other conſequences already explained. The pungent and ſtimulating quality of his perſpirable matter will promote theſe ſalutary effects. In this view of the ſubject, it may be worthy of inveſtigation, whether the practice of the ancients of uſing unguents on the ſkin, ſtill general among the eaſtern nations, and common among all ſavages, whoſe defective cloathing obliges them to guard by this means againſt the viciſſitudes of temperature, ſhould not be revived by our countrymen in the Weſt Indies. It has been imagined, that thoſe unguents effectually obſtructed the perſpiration; but this notion has been adopted without inquiry, and againſt probability. Theſe unguents, like the perſpirable matter of the negro, may be formed of a conſiſtence, that while it retards exceſſive ſweating, does not obſtruct moderate and neceſſary perſpiration, and being themſelves evaporable, they may keep up a coolneſs that ſhall diminiſh the neceſſity of the natural diſcharge. In the warm climates the tepid bath ſeems a neceſſary adjunct to inunction. It has been for ſome time uſed among the French in the Weſt Indies, and is now, (as we are informed) beginning to be adopted by the Engliſh; and with proper reſtrictions, it may produce the happieſt effects. After exerciſe, under a burning ſun, when the ſtrength is waſted by fatigue and perſpiration, immerſion in the bath heated from 90° to 95° of Faht. will be found moſt grateful and ſalutary. It reſtrains the profuſe and colliquative ſweating, keeps up the heat of the ſurface and extremities, and prevents the dangerous re-action of the centre, which a torpor of the extremities ſo generally produces. It allays the violent action of the heart and arteries, and ſooths the ſyſtem of ſenſation. On leaving the bath, friction, and inunction of the ſurface prevent the chill of evaporation, and while they enſure the ſafety of the practice, they prolong this claſſic gratification. The refinement of modern manners has introduced linen teguments next the body, and aboliſhed inunctions of the ſurface. Hence the warm bath has become leſs ſafe, as opening the pores of the ſkin, and expoſing the naked body to the chills of evaporation; and thus through the greater part of Europe it no longer forms a part of the regimen of health. In the eaſt it ſtill prevails. If it ſhould be revived among our countrymen in the weſt, they muſt conjoin with it the inunctions of the ſurface, and the fine flannel teguments next the ſkin, which accompanied its uſe among the Greeks and Romans. In recommending a trial of unguents to the enlightened practitioners of the torrid zone, I may ſhelter myſelf under the authority of the celebrated Lord Verulam, unqueſtionably the firſt phyſician, as well as the firſt philoſopher and ſtateſman of the century in which

he lived. " *Inunctio ex oleo, et hyeme confert ad ſanitatem, per excluſionem frigoris, et æſtate, ad detinendos ſpiritus, et prohibendam exolutionem eorum, et arcendam vim aeris, quæ tunc maxime eſt prædatoria. Ante omnia igitur uſum olei vel olivarum vel amygdali dulcis, ad cutem ab extra unguendam, ad longævitatem conducere exiſtimamus.*[*]

The operation of unguents on the ſurface of the body, preſents indeed a ſubject for important and original obſervation. The effects of the warm and tepid bath, though more inveſtigated, are ſcarcely better underſtood; for perhaps there is no part of medicine on which ſo much has been written, and ſo little been decided. Theſe ſubjects are connected together, and might form, with great advantage, part of an experimental inquiry into the laws of animal heat. The commonly received opinion, that the warm bath relaxes and enfeebles the ſyſtem, muſt, I apprehend, be admitted with many reſtrictions. Immerſed in water, or in air, heated to the degree that quickens the circulation, we are doubtleſs ſpeedily enfeebled. But by a heat ſhort of this effect, it may be diſputed whether debility is ever produced. The degree to which the bath muſt be heated in order to quicken the circulation, approaches nearly to that of the blood. In my own caſe, I have found the pulſe became more frequent at 96°. when the ſtomach was empty, but at 94° after dinner; and the practice ſaid to have been adopted by the ancients of going into the hot bath to relieve themſelves from the oppreſſion of an over-loaded ſtomach, would appear to me to have been attended with ſome hazard, and with great inconvenience. It does not indeed appear how the deſired effect was obtained, except through the means of profuſe perſpirations. In future experiments reſpecting the effects of the warm bath, it will be of importance to notice the heat at which the ſenſible perſpiration begins to flow; which probably varies a good deal in different conſtitutions, and which as it varies, will materially affect the reſults. It will be important alſo to notice the ſtate of the ſtomach as to fullneſs; the condition of the pulſe; the previous degree of exerciſe; and the actual heat of the ſurface; all of which, according to my obſervation, will influence the experiments.

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[*] *See Fran. Bacon. Opera fol. Francofurti ad Moenum*, 1665. *p*. 536, 537. 538, where the practice of various nations reſpecting unguents, is conſidered at ſome length, and the cautions requiſite in adopting this practice, leaſt the body ſhould be overheated, &c. are very ingeniouſly detailed.

In all inflammatory diſeaſes, it is of importance not to uſe the bath heated to the degree that materially quickens the circulation: where this is not attended to, the ſymptoms are inflamed, unleſs a ſpeedy and profuſe perſpiration enſues. In the degrees in which it does not quicken the circulation, the warm bath is ſoothing and ſedative, eſpecially when the immerſion is prolonged; and it is the temperature from 95°. to 90°. that is ſo ſingularly reſtorative after fatigue, though a ſtill lower heat is ſafe and refreſhing, as thoſe who have uſed the baths of Buxton can teſtify.—The warm bath is frequently employed to excite a ſenſible perſpiration, which may be prolonged after leaving it; and in this way it may be uſed with great advantage. Where this object is in view, it is adviſable to immerſe the patient in the water heated to 94°. or 95°. and very gradually and ſlowly to increaſe the heat to 97°. or perhaps 98°, watching its effects. When the ſweat begins to appear on the forehead, if the pulſe remains calm, and the patient feels at eaſe, an increaſe of heat to 99°. or even 100°. may be ventured on with ſafety, if profuſe perſpiration be required. But when the bath is heated *in the firſt inſtance to* 100°. the ſtimulus of heat produces a feveriſh circulation, which the ſubſequent imperfect perſpiration cannot allay. The injurious effects are ſtill greater where the bath is heated at firſt to 105°. or upwards, and ſweat, inſtead of flowing more freely in conſequence of the increaſed temperature, is diſcharged with greater difficulty, the ſtimulus of the heat, and the ſuddenneſs of its application, inducing a contraction of the orifices of the extreme veſſels of the ſkin. In this reſpect, as in moſt others, the analogy is perfect between the ſtomach and the ſurface; for it will uniformly be found, that perſpiration is excited more freely by draughts of *tepid* liquor, than an equal quantity of the ſame fluid ſwallowed as hot as it can be borne.

The ſenſible perſpiration excited in the warm bath, does not lower the temperature of the body while immerſed, but being prolonged afterwards, it becomes powerfully refrigerant, and is a remedy of great efficacy. But theſe obſervations ſhew the importance of regulating the temperature of the warm bath by the thermometer, and demonſtrate how little dependance can be placed on the obſervations of thoſe writers who ſpeak of its effects without noting its temperature. A variation of two or three degrees, often impoſſible to be aſcertained by the ſenſations, will not merely vary the degree, but alter the nature of its effects.

The view given of the nature of fever, and of the functions of the perſpiration, ſeems to afford an obvious explanation of the operation of thoſe antimonial emetics, and ſudorifics, by the early exhibition of which, fevers are often ſtopped in their firſt ſtages. The direct action of theſe medicines on the ſtomach reſolves the ſpaſmodic ſtricture of the capillaries of that organ and of the ſurface, by which the morbid heat is confined; while the concuſſion given to the whole ſyſtem, diſſolves the morbid catenation, and terminates the diſeaſe. Theſe remedies are however uncertain in their beneficial effects, and always highly debilitating. They cannot enter into competition with the affuſion of cold water on the ſurface, but may be employed when the preſence of local inflammation precludes the uſe of that powerful remedy.

Theſe general views caſt ſome light alſo on that moſt curious ſubject, the operation of opium on the living principle. In former times it was diſputed whether opium was in its nature cold or hot; in our own days it has been diſputed whether it is ſtimulant or ſedative. Such abſtractions afford exerciſe for the ingenuity, but in the way in which they have been conducted, they ſoar above utility and truth. Opium relieves pain, and produces ſleep, and theſe effects being in general concomitant, may be treated of together. But though opium in general produces ſleep, yet we are ſometimes diſappointed in this effect, and eſpecially in thoſe feveriſh diſorders where ſleep is of ſuch high importance. About nine years ago, I attended a boy of ten years of age, the ſon of a friend, dangerouſly ill of fever. His nights had been ſleepleſs, and the general irritation was very great. Opium had been adminiſtered in vain. I determined to give it in larger quantities, and to watch its effects. At ten at night he took forty drops of laudanum; at twelve the ſame doſe was repeated. As I ſtood by his bed two hours afterwards, he appeared in an imperfect and agitated ſleep: his eyes were half open, his face was livid; his lips and ſkin parched; he was evidently much diſturbed, and moaned in his breathing. At this time I perceived that he had toſſed the cloaths off his feet, which lay naked; and that he graſped the cold bed-poſt with one of his hands. On examining his hands and feet they felt dry and burning. There was tepid vinegar in the room, and I moiſtened firſt his hands, and then his feet with it, without awaking him, repeating this practice from time to time: he became more quiet. Thus encouraged, I gradually moiſtened his legs and thighs in the ſame manner, and at length his breaſt and neck, and removing the reſt of the bed-clothes, left him covered with a ſheet only. In a little while the agitation ſubſided, and he ſunk into a ſound, and ſeemingly peaceful ſleep, which continued ſeveral hours, the ſurface of his body being bedewed with a gentle perſpiration.

This intereſting caſe pointed out to me clearly that the ſedative effects of opium are often counteracted by the ſtimulus of heat on the ſurface and extremities, and that the actual ſtate of the heat of the patient is a circumſtance requiring particular attention in the adminiſtration of this powerful remedy. The following obſervations are the reſult of the inquiry which theſe reflections ſuggeſted.

Opium given in health generally produces ſleep, *if the mind be vacant, the ſtomach empty, and external impreſſions be excluded.* In this caſe, its firſt effect is to increaſe the ſenſibility, to give a gaiety and livelineſs to the imagination, and to diffuſe a genial glow over the ſurface and extremities. The actual heat of the trunk of the body is little if at all increaſed, but the ſurface and extremities are brought up to the general temperature, (97° or 98°) and a gentle, and as it has been called a breathing perſpiration is diffuſed over the ſkin. In this ſtate we ſink into thoſe happy ſlumbers which are ill exchanged for the realities of life. On the approach of ſleep, the pulſe is generally increaſed in frequency by four or five pulſations in the minute, and the reſpiration is ſlightly irregular, as in the gentler degrees of pleaſurable emotion; but as the ſleep becomes fixed and profound, the pulſe ſubſides to its ſloweſt and moſt tranquil ſtate, and the reſpiration becomes regular, deep, and rather ſlower than natural. The various circumſtances that prevent this favourable operation of opium, even in the ſtate of health, it is not our purpoſe to conſider at preſent.

When opium is given in fever, if the heat be two or three degrees or upwards above the natural ſtandard, and the ſkin dry, it ſeems very generally to increaſe the heat and reſtleſſneſs. There are exceptions. If the heat, though præternaturally great, is ſubſiding, and the ſkin beginning to ſoften, though not yet moiſt, opium very often accelerates the perſpiration, and by this means diminiſhes the heat. In ſuch caſes its ſalutary effects generally follow—tranquillity and ſleep. Thus it will happen, that an anodyne draught given early in the evening ſhall occaſion increaſed heat and agitation, which if deferred till two or three in the morning would have produced perſpiration and repoſe. In the evening the exacerbation of fever is on the increaſe or at its height, which towards morning is ſubſiding, the difference in the actual heat of the ſurface being often not leſs than two degrees or upwards. A doſe of opium given in the hot ſtage of intermittent fever, will often accelerate the ſweating ſtage and ſhorten the paroxyſm, which under ſimilar circumſtances in continued fever, has a contrary effect; becauſe in the former caſe the diſpoſition to perſpire is more eaſily excited. But even in intermittent fever, opium when given in the hot ſtage, will be much promoted in its diaphoretic and ſalutary effects, by moderate draughts of tepid, or if the heat be great, of cold liquids. In continued fever where the heat is great and the ſkin dry, it is proper to lower the temperature of the ſurface, and if poſſible to excite ſenſible perſpiration before opium is adminiſtered, if we wiſh to enſure its diaphoretic and ſoporific effects. But even after opium has been exhibited, when the inordinate heat prevents its ſedative operation, it will be found ſafe and ſalutary to uſe the tepid or cold affuſion; and when the heat is by this means reduced, repoſe and ſleep will follow. Tepid or cold drink will produce, though in a weaker degree, ſimilar benefit. Theſe methods of promoting the diaphoretic effects of opium, ſeem more certain and advantageous in fever, than the practice of combining it with ipecacuanha or the preparations of antimony; but where opium is to be uſed in inflammatory diſeaſes, or in dyſentery, doubtleſs this laſt method is to be preferred. Theſe remarks muſt be conſidered as applying to opium in its ordinary doſes, that is, from half a grain to two or three grains of the extract, or from ten to ſixty drops of the tincture. Perhaps it has been too much the practice of late to give this medicine in large doſes, and to overlook its effects in ſmaller quantities. Experience has convinced me that conſiderable effects are produced on the ſyſtem by a very few drops of the tincture properly adminiſtered, and that it is always unwiſe to employ it in doſes larger than neceſſary to produce the deſired effect.

The effects of alcohol (under which term I comprehend the ſpirituous and vinous liquors) on the ſyſtem, have a ſtriking reſemblance to thoſe of opium. Alcohol is more heating, leſs diaphoretic, and leſs ſoporific. When however *the mind is vacant*, *the ſtomach empty*, and *external impreſſions excluded*, alcohol like opium has a tendency to induce ſleep. As ſleep however approaches, the heat of the body riſes, eſpecially on the ſurface and extremities, and ſtimulating the heart and arteries to increaſed action, produces a ſtate of agitation with which ſleep is often incompatible. Or if the doſe has been ſuch as to benumb the ſenſes, with the firſt return of ſenſibility the inebriate is rouſed by intolerable heat from his apoplectic ſlumbers. The degree of heat in the fever of intoxication is doubtleſs various in various conſtitutions. I have obſerved it to riſe to 105°, nearly the higheſt degree of heat in the typhus of this iſland; and Dr. Alexander in his own caſe, found it as high as 107°.—Under theſe degrees of heat, the thirſt, agitation, and conſequent debility are very conſiderable, the ſkin being in general obſtinately conſtricted. The proper treatment in this, as perhaps in all other caſes of morbid heat and dry ſkin, when local inflammation is abſent, is to pour large draughts of cold water into the ſtomach, or to affuſe it on the ſurface. By this practice the heat is reduced, the orifices of the ſkin opened, ſenſible perſpiration is produced, and one of half the miſchief of intoxication is prevented. Similar effects may be produced, though in a weaker degree, by tepid drinks or the tepid affuſion.

The adminiſtration of alcohol in fever requires ſimilar precautions as that of opium. It ſhould not be given when the heat is great and the ſkin dry and burning; but it may in general be given with great ſafety and advantage, when the heat is nearly of the natural ſtandard; and even when it is ſomewhat above it, if the orifices of the ſkin are open and the ſurface is moiſt. Opium, as was before remarked, is more ſudorific than alcohol, and given in that ſtage of the fever of intoxication in which the ſkin begins to relax, it often accelerates ſenſible perſpiration, and ſometimes occaſions it, when it might not otherwiſe have taken place. In this way I explain a phenomenon which has occurred to others as well as to me, that a moderate doſe of opium ſometimes gives extraordinary relief to the drunkard, under the ſufferings through which he muſt paſs to ſanity and ſtrength.

Theſe obſervations are not offered to the reader as including a complete view of the ſubject of which they treat. My principal object has been to eſtabliſh the uſe of a new and powerful remedy in fever, and the general remarks and reaſonings which are added, are chiefly with a view to explain its operation and enforce it ſafety. As however theſe additional obſervations are on ſubjects of great importance, it is my intention at ſome future period to reſume them. In the mean time, it may be objected that in this rapid ſketch aſſertions are ſometimes given inſtead of experiments and proofs. In ſuch caſes however, it will be found that the points as iſſue may be readily brought to the teſt of experiment; and as general and obſcure expreſſions are every where avoided, it will be eaſy to reſert to this trial; a circumſtance which will procure from candid minds a patient hearing, as well as forgiveneſs for thoſe errors into which I may have involuntarily fallen.

I have only to add, that the application of cold under any form, where fever is combined with local inflammation, I have purpoſely avoided to diſcuſs. It is a ſubject of much difficulty, and muſt wait for the elucidations of future experience.

CHAP. XVI.

Population of Liverpool.—Prevalence of fever among the poor, with documents.—Benefit derived from the Mancheſter Houſe of Recovery in contagious diſeaſes.—Propriety of the general adoption of ſimilar eſtabliſhments.

THE Typhus, or low contagious fever, prevails in all large cities and towns to a degree that thoſe are not aware of, who have not turned their attention to the ſubject, or whoſe occupations do not lead them to mix with the labouring poor. In Liverpool it has been ſuppoſed that this diſeaſe is ſeldom to be met with, and it is certainly true, that the upper claſſes of the inhabitants are not often ſubjected to its ravages.* It has never, in the laſt ſixteen

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* "Such is the general healthy ſtate of the town, that infectious fevers are *never known to prevail*, and it is very rare to hear of a perſon dying of a fever of any ſort."
Liverpool Guide, p. 134.

ſixteen years, ſpread among the higher claſſes ſo as to occaſion any general alarm; and when the extent to which it is conſtantly preſent among the poor, ſhall be proved by authentic documents, this circumſtance will ſerve to demonſtrate the narrow ſphere of the contagion, and to ſhew how much it is within the limits of human power to extirpate the diſeaſe. Before we produce theſe documents, it will be proper to offer a few general obſervations.

The population of Liverpool, as taken by actual enumeration, amounted on the firſt day of January 1790, to nearly 56,000 perſons, and admitting the increaſe ſince that time to be in the ratio of preceding years, it would now (October 1797) exceed 63,000. But as the progreſs of the town has doubtleſs been checked by the war, ſomething may be deducted from this laſt number; though we ſhall probably be under the truth in fixing it as low as 60,000.* Of the inhabitants of Liverpool, it is aſcertained, that about 7,000 live in cellars under ground, and nearly 9,000 in back houſes, which in general have an imperfect ventilation, eſpecially in the new ſtreets on the ſouth ſide of the town, where a pernicious practice has lately been introduced of building houſes to be let to labourers, in ſmall confined courts, which have a communication with the ſtreet by a narrow aperture, but no paſſage for the air through them. Among the inhabitants of the cellars, and of theſe back houſes, the

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* The enumeration mentioned, was made by Mr. Makin Simmons, and publiſhed in Mr. Gore's directory in the year 1790, but omitted in the laſt edition. It occupied him three months, viz. from October 13th, 1789, to January 13th, 1790, and was executed with great care and accuracy. Mr. Simmons gave the population of each ſtreet ſeparately, enumerating alſo the front houſes, back houſes or cottages, and the

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inhabited cellars, with the number of perſons in each of theſe deſcriptions of habitation. The general reſult was as follows.

Front houſes	6540,	containing	39,118 Inhabitants.
Back houſes or Cottages	1608,	containing	7,955 Inhabitants.
Inhabited Cellars	1728,	containing	6,780 Inhabitants.
The Workhouſe and different Charities,	1,879		
			In all, 55,732.

There were beſides empty houſes	717,	
Making in all	10,593,	Houſes, or if the Cellars be
Deducted	1,728,	8865, will be the number.

There is no town of an equal ſize in the kingdom that poſſeſſes (as far as I have heard) ſo authentic a document of its population. The numeration of Mr. Simmons fell ſhort of what many ſuppoſed to be the number of our inhabitants; but popular calculations on this ſubject always exceed the truth—perhaps not leſs in general than by a fourth part. The popular calculation of the inhabitants of Liverpool, compared with other towns of which there is no accurate enumeration, may be taken at 75,000.

the typhus is conſtantly preſent, and the number of perſons under this diſeaſe that apply for medical aſſiſtance to the charitable inſtitutions, the public will be aſtoniſhed to hear, exceeds 3,000 annually.*

The public charitable inſtitutions in Liverpool for the relief of the ſick poor, are the Infirmary and the Diſpenſary. Fevers, by one of the original rules of the Infirmary, are excluded from that hoſpital, though this rule, as may be ſeen from the narrative with which this little volume commences, has been occaſionally departed from. But with a few exceptions, the care of the poor in fever falls on the Diſpenſary. Of this inſtitution I was for ſeveral years one of the phyſicians, and ſuch of my obſervations reſpecting it as are not ſupported by authentic documents, are founded on perſonal obſervation. Unleſs in caſes where they are removed into the Infirmary, the care of almoſt all the inhabitants of cellars when ſick, and of a great part of the inhabitants of the back houſes or cottages, devolves on the phyſicians, ſurgeons, and apothecaries of the Diſpenſary. This deſcription of perſons, amounting in all to about 16,000, do not appear to be healthy, the number of caſes admitted annually on the books of the Diſpenſary, on the average of the laſt ten years, exceeding 13,000. In the commencement of the year 1780, a regiſter of theſe caſes was begun by Mr. Aviſon, late apothecary to this charity, and continued by him to the ſummer of the preſent year, when he terminated a life of ſpotleſs integrity, ſingular uſefulneſs, and inceſſant exertion*. From this valuable record I have formed the following table.

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* From the firſt of January 1787 to the laſt of December 1796 incluſive, a ſpace of ten years, it will appear by the table which follows, that 31,243 caſes of fever were admitted on the books of the Diſpenſary alone, which on an average, is 3124 yearly. If it be ſuppoſed that ſome caſes may be denominated typhus by miſtake, let it be conſidered, how many caſes of this diſeaſe do not appear on the books of the Diſpenſary, though occurring among the poor, being attended by the ſurgeons and apothecaries of the benefit clubs to which they belong, &c.

GENERAL

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* I inſert the diſeaſes of September 1790, extracted from the Regiſter, which at the ſame time will convey a ſpecimen of our moſt prevailing diſeaſes, and ſhew that inflammatory and ſymptomatic fevers are not included under the general title of fever.

September 1790.

Febris	310	Catarrhus	24	Debilitas	19
Ophthalmia	59	Dyſenteria	2	Hydrops	8
Cynanche	13	Cephalalgia	10	Scrophula	11
Pneumonia	28	Apoplexia		Rachitis	
Hepatitis		Paralyſis	4	Syphilis	24
Gaſtritis		Dyſpepſia	21	Icterus	1
Phthiſis	19	Epilepſia	1	Dyſuria	5
Rheumatiſmus	52	Aſthma	16	Vermes	5
Eryſipelas	8	Pertuſſis	11	Pſora	142
Variola		Colica	24	Partus Difficilis	1
Rubeola	29	Cholera	24	Uſtio	14
Scarlatina		Diarhœa	24	Ulcus	61
Aphtha	13	Hyſteria	4	Vulnus	89
Hœmorrhois	3	Convulſio	2		
Hœmorrhagia	4	Inſania	1	Total	1093

GENERAL TABLE,

Shewing the number of Patients admitted on the books of the Liverpool Diſpenſary from the firſt of Jan. 1780, *till the laſt of Dec.* 1796, *a ſpace of* 17 *years, diſtinguiſhing the numbers of each year, and of each month of every year.*

Year	*Jan.*	*Feb.*	*Mch.*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct*	*Nov.*	*Dec.*	*Total.*
1780	890	888	1147	995	827	805	787	672	557	825	730	855	9978
1781	893	755	1074	1028	940	846	951	889	981	1212	1081	1099	11749
1782	1106	1041	1150	1243	811	1605	686	619	722	736	704	760	11183
1783	975	858	1199	1232	1096	1035	1064	1062	1050	1094	992	966	12623
1784	738	915	976	969	968	934	895	877	929	934	1055	1009	11199
1785	1092	1075	1124	1082	992	1015	856	850	1005	1057	882	885	11915
1786	982	760	1117	969	986	791	816	1010	930	895	858	959	11100
1787	1092	881	1060	1015	1041	964	820	821	868	848	958	900	11268
1788	1022	960	882	1002	944	903	916	1115	1009	927	951	1140	11771
1789	1415	1053	1044	1153	1113	1070	1123	930	1017	908	982	1095	12903
1790	947	1052	1361	1123	1152	1098	1039	1001	1093	1175	1162	1034	13237
1791	1223	1126	1323	1118	1176	1166	1091	1112	1241	1257	1274	1093	14200
1792	1307	1302	1427	1277	1159	1353	1106	1013	985	1343	1143	1030	14444
1793	1088	1037	1188	1228	1247	1065	1077	1065	1128	1335	1215	1159	13832
1794	1051	1175	1170	1138	1112	1105	1066	1089	1160	1022	1306	1273	13667
1795	1298	1369	2003	1171	1209	1151	1266	945	1064	1259	1300	1200	15235
1796	1120	1141	1230	1402	1030	988	948	918	934	1087	967	1236	13001
	18239	17388	20475	19172	17803	17894	16507	15988	16673	17923	17560	17093	213,395

By the foregoing table it appears, that the number of patients admitted under the care of the Diſpenſary annually, taking the average of the laſt ſeventeen years, is 12,548. But if the laſt ten years only be taken, the average number will be found to be 13,355. It appears that the greateſt number of patients admitted in any one year, was in 1795, amounting to 15,235, exceeding the average of the laſt ten years by 1880; and that the ſmalleſt number admitted for the laſt ſeven years, was in 1796, falling ſhort of that average by 354, and of the number admitted in 1795, by 2234. When it is conſidered that the difference between theſe two years falls chiefly in the winter and ſpring months; that the winter and ſpring of 1795 were the coldeſt, and thoſe of 1796 the mildeſt, experienced in our climate for a long ſeries of years; theſe ſtriking facts corroborate the ſtatements of Dr. Heberden, in proving that the common opinion, which ſuppoſes a mild winter in our iſland to be unhealthy, and a ſevere winter to be favourable to health, is in both particulars the reverſe of the truth. It appears alſo, that though the number of patients in the winter months of 1794-5, greatly exceeded the general average, yet it was in February, and more eſpecially in March, that the exceſs roſe ſo very high; on this laſt month the aſtoniſhing number of 2003 patients having been admitted. In the three preceding months the cold had been intenſe, but in March the weather became warmer, uncertain, and ſtormy. Hence it appears, that though intenſe cold is directly unfavourable to the health of our poor, yet that it is in the tranſition from this intenſe cold to warmer weather, that their conſtitutions ſuſtain the ſevereſt ſhock; a poſition that is ſtill more fully ſupported by the deciſive evidence that the month of March is the moſt unhealthy of the year, and next to it the month of April, taking the average of ſeventeen years together. On the ſame evidence we may ſtate the ſucceſſion of the months in reſpect to their unhealthineſs in the following order—*March*—*April*—*January*—*October*—*May*—*December*—*November*—*February*—*June*—*September*—*July*—and *Auguſt*.* But though on the whole, ſummer and autumn are more healthy than winter and ſpring, the difference is perhaps leſs than might have been expected; and the difference even between the two months that forms the extremes, is not very great. In the ſeventeen years recorded in the table, the average number of patients for each month of the whole is 1045; the average number in the month of March is 1204; in Auguſt 940. Theſe obſervations however are ſtrictly applicable only to a particular deſcription of perſons in a particular ſituation—to the poor of Liverpool—and without the corroboration of other documents, muſt not be extended to the general maſs of ſociety, or to the kingdom at large.

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* It appears that June ſtands in the table higher than May or December, but this ariſes from the vaſt number admitted in June 1782, when the influenza was prevalent, and was confined nearly to that month. Allowance being made for this, June will ſtand as inſerted.

I proceed to give, from the ſame authentic regiſter, the table of typhus, the more immediate object of our inquiries.

A TABLE,

Shewing the number of Fevers admitted on the books of the Liverpool Diſpenſary from the firſt of January 1780, to the laſt of Dec. 1796, incluſive; diſtinguiſhing the numbers admitted every year, and each month of every year.

Year	Jan.	Feb.	Mch.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total.
1780	150	125	179	173	168	183	191	150	129	186	150	133	1917
1781	130	146	180	200	187	154	157	127	167	234	208	223	2113
1782	268	265	231	292	148	159	120	140	143	182	150	158	2256
1783	210	158	184	207	222	212	286	227	265	316	257	273	2817
1784	170	194	245	247	232	225	270	230	266	247	369	297	2992
1785	285	268	296	294	219	187	173	180	186	250	244	182	2764
1786	191	166	216	244	202	155	159	188	169	211	167	197	2265
1787	256	209	301	234	313	356	255	192	218	234	283	326	3177
1788	236	174	213	255	253	245	271	311	258	341	315	295	3167
1789	319	176	338	323	391	205	184	162	212	214	204	208	2936
1790	176	248	337	294	281	247	343	270	310	340	355	269	3470
1791	253	247	277	230	233	240	266	248	300	344	335	371	3344
1792	359	361	269	278	261	237	236	223	211	330	212	174	3151
1793	174	209	221	259	237	334	199	197	338	305	224	228	2925
1794	157	230	383	280	337	305	291	245	303	290	258	326	3405
1795	152	265	546	204	234	230	248	159	196	239	317	180	2970
1796	197	161	266	242	288	176	203	182	254	329	153	247	2698
	3683	3602	4682	4256	4206	3850	3852	3431	4025	4592	4201	4047	48367

Thus it appears, that of 213,305 patients admitted in ſeventeen years at the Diſpenſary, 48,367 have laboured under typhus; nearly a fourth of the whole; an immenſe proportion! It is curious to obſerve, that March takes the lead in reſpect to the prevalence of fever, as well as of diſeaſes in general, and that Auguſt, as before, brings up the rear. It appears alſo that the month of March 1795, which exceeded every other month in the production of diſeaſes in general, exceeds in a ſtill greater proportion in the production of fever; no leſs than 546 patients, having been admitted in that month under this ſingle diſeaſe. The *year* 1795 has not however the ſame unfortunate pre-eminence here as in the table of general diſeaſe, being ſurpaſſed in the number of fevers by ſeveral others. But the year 1796 enjoys a ſimilar diſtinction as formerly, having produced fewer fevers than any other of the laſt ten, and 545 fewer than the average of the whole. Taking the ſeventeen years in the table, the average number of fevers annually is 2845, and the average number monthly is 237. The average number of March riſes to 275, and of Auguſt, ſinks to 202. On the whole the prevalence of fever is greater, and the influence of ſeaſons upon it leſs, than might have been expected.

For the laſt ten years there have been, on an average, 119 patients ill of fever conſtantly on the books of the Diſpenſary; or if the laſt ſeventeen years be taken, the conſtant average number will be 109. Of convaleſcents, unfit for labour, the average number will be nearly as great. Thus, in Liverpool, 240 of the poor may be conſidered as conſtantly rendered incapable of earning their ſubſiſtence by this ſingle diſeaſe; and as the poor ſeldom lay up any part of their earnings for a ſeaſon of ſickneſs, the expenſe of their maintenance muſt, in one form or other, fall on the public. If we take this as low as 10l. for each, it will amount to 2400l. annually.*

It would afford me particular pleaſure to introduce authentic documents of the ſucceſs attending the treatment of the typhus by my brethren of the Diſpenſary; but the valuable regiſter which furniſhed materials for the preceding tables does not mark the iſſue of the particular caſes. In general however it may be aſſerted, that where the aſſiſtance of the Diſpenſary is reſorted to in time, even under the diſadvantage of being attended in confined and dirty habitations, a great proportion of caſes terminate favourably. The general treatment of fever at the Diſpenſary, conſiſts in giving antimonial emetics in the firſt inſtance, and afterwards bark, opium, and wine. Nouriſhing food is occaſionally adminiſtered. Ablution with cold water has ſeldom been uſed, and indeed it is in general difficult, if not impoſſible, to employ it in the obſcure cellars in which this diſeaſe is uſually found. The fever that prevails among our poor is remarkably uniform; it is the pure typhus, to which the cordial treatment can be applied with ſafety, and to a great extent. Whereas among the higher claſſes, fever is often attended with inflammatory ſymptoms in the firſt inſtance, ſometimes with pneumonic ſymptoms through a conſiderable part of the diſeaſe; and in ſuch caſes the indications of practice being contradictory, ſucceſs is much leſs certain.

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* Theſe calculations will not be expected to be minutely correct, but I apprehend they will in general be found under the truth. The average number of fevers annually for the laſt ten years, is 3124, and the mean duration of each fever is ſuppoſed to be fourteen days; $\frac{3124 \times 14}{365} = 119.8$ daily. If the average of the laſt ſeventeen years be taken, this will give 2845 annually, which produces on the ſame principles 109 daily. I calculate that the period of convaleſcence after each fever is fourteen days, or upwards, which gives the ſame number of convaleſcents as of patients in fever, deducting thoſe who may die, a much ſmaller proportion than could well be ſuppoſed.

Though the cure of this diſeaſe is a principal object of our charitable inſtitutions in Liverpool, it is to be lamented that little or nothing has been done for its prevention; although it may be confidently aſſerted, that this object, great as it is, is not beyond the reach of human power.— This infection ariſes from a want of cleanlineſs, and ventilation; and its influence is promoted by damp, fatigue, ſorrow, and hunger. When the ſubject ſhall be ſeriouſly taken up by the public, the means of ſecuring cleanlineſs and ventilation even in the greater part of our ſubterraneous dwellings, may be clearly pointed out, and though the ſecondary cauſes, if I may ſo call them, ſeem leſs capable of being removed, yet much may be done to alleviate them, when a comprehenſive inquiry into the condition and wants of the poor (the moſt preſſing of all objects in the eye of enlightened patriotiſm) ſhall engage the attention of the legiſlature.*

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* The want of a diet ſufficiently nutritious, is doubtleſs one of the cauſes that promote the typhus, and other diſeaſes, among our poor. This does not ſeem to ariſe in general from the price of their labour being inadequate to furniſh ſuch a diet, but from their ignorance in the moſt advantageous modes of cookery, and ſtill more from their indulging in articles that conſume their means without adding to their ſuſtenance. In the eighteen hundred cellars in Liverpool, there are many in which animal food is not taſted more than once a week; but there are very few in which tea is not drank daily; it is often indeed drank twice a day. The money ſpent on tea is worſe than waſted. It is not only deverted to an article that furniſhes no nutrition, but to one that debilitates the empty ſtomach, and incapacitates for labour. Hence the vaſt number of dyſphetic complaints among our patients

Important information in regard to the means of preventing the generation and progreſs of contagion, may be found in the writings of phyſicians of our own county and neighbourhood; of Dr. Percival and Dr. Ferriar of Mancheſter, of Dr. Campbell of Lancaſter, and of Dr. Haygarth of Cheſter. The perſevering exertions of Dr. Percival, Dr. Ferriar, and the other gentlemen of the faculty attending the Mancheſter Infirmary, have produced an inſtitution in that town, which deſerves to be imitated in every large city and town in the kingdom; a houſe of reception, or as it is juſtly called, a Houſe of Recovery for the poor, when labouring under fever. Into this aſylum, on the firſt notice of the diſeaſe, the patient is immediately removed, and proper methods taken to purify his habitation. By theſe means the contagion is extinguiſhed in its birth; and the patient himſelf being removed to a ſituation where all the reſources of art can be employed with advantage for his benefit, and removed at a period of the diſeaſe when theſe reſources are of great avail, is very generally reſtored in health to his family and to the community. The

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at the public charities, which are almoſt all to be traced to the uſe of tea or ſpirits, often indeed aſſiſted by depreſſion of mind. At the Infirmary and Diſpenſary together, this claſs of patients exceeds five hundred annually. The great majority are females.

The benefits of this admirable inſtitution in Mancheſter, no longer reſt on the baſis of theory; they are eſtabliſhed on the immoveable foundation of fact and experience. The prevalence of fever in the town is diminiſhed to a degree that has exceeded all rational expectation; and the fears entertained, that the Houſe of Recovery might ſpread the contagion through the ſtreets in its vicinity, are found to be entirely groundleſs.*

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* The following facts are extracted from the report of the *Board of Health*, dated *Mancheſter*, *May* 26*th*, 1797. Since the eſtabliſhment of the *Houſe of Recovery*, the whole number of home patients (i. e. patients requiring attendance at their own habitations) is reduced *one half*; but the number of fever caſes, in a far greater proportion. In January 1796, 226 caſes of fever were admitted at the Infirmary of Mancheſter, but in January 1797, only 57. Fears had been entertained that the fever might ſpread from the Houſe of Recovery into the neighbouring buildings, and ſo through the town, and this had given riſe to a conſiderable oppoſition. The ſtreets in the immediate vicinity of the Houſe of Recovery, are Silver-ſtreet, Portland-ſtreet, &c. In the firſt eight months *after* the eſtabliſhment of the Houſe of Recovery, the number of caſes of fever admitted on the books of the Infirmary from theſe ſtreets was 25. In eight months of the preceding year, viz. from the 20th of September 1795, to the 20th of May 1796, the ſame ſtreets furniſhed 267 caſes of fever. From the 20th of Sept. 1794 to the 20th of May 1795, they furniſhed 389 caſes; and from the 20th of Sept. 1793 to the 20th of May 1794, 400 caſes. Facts ſuch as theſe, require no comment; they have made a forcible impreſſion in Mancheſter, as

The utility of the Houſe of Recovery in Mancheſter has been ſtill more completely aſcertained in the courſe of the preſent ſummer.—“ Within “ this laſt four months (I copy a paragraph in a letter from Dr. Ferriar) “ we have been threatened “ with the return of an epidemic fever, and dan- “ gerous caſes of typhus have appeared in every “ quarter of the town; yet by ſelecting thoſe pa- “ tients who were, from their ſymptoms and ſitua-

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might be expected, and the Report I quote contains public teſtimonies in favour of this excellent Inſtitution from the *Board* of the *Infirmary*, the *Stranger's Friend Society*, and *The Special Board of Overſeers of the Poor*. The total number of fever-caſes received into the Houſe of Recovery from its being opened, the 19th of May 1796, to the 31ſt of May 1797, is, 371

Of theſe are diſcharged cured,	324
Dead,	40
Remaining,	7
	371.

The benefit of this inſtitution is not to be calculated by the number of perſons cured. Every ſingle removal into the Houſe of Recovery, probably prevents on an average two or three caſes of the diſeaſe. As the inſtitution goes on, it may be confidently predicted, that the proportion of cures will be ſtill greater than here ſtated, becauſe experience will induce the poor to apply for relief in the early ſtages of fever, when the power of medicine may be exerted with the greateſt advantage. I have the authority of Dr. Ferriar for ſaying, that ſince the 31ſt of May to the 25th of September, two perſons only have died in the Houſe of Recovery.

" tion, moſt likely to communicate infection, and " by removing them into the fever-ward, the weekly liſt of home-patients has been prevented from " increaſing much beyond their uſual number. " Our Houſe of Recovery, which contains accommodations for twenty-eight patients, is ſufficient " for Mancheſter at preſent. In the event of a " peace, and the influx of new inhabitants which " it muſt occaſion, it may be neceſſary to provide " accommodation for a greater number. But I " think fever-wards capable of containing fifty patients, with ſeparate rooms for the reception of " caſes of *Scarletina Anginoſa*, *Meaſles*, or *Small Pox*, " would anſwer every purpoſe here, in times of the " greateſt emergency, even ſuppoſing a very conſiderable increaſe of population."

'Till the latter end of the year 1787, there was no place for the reception of fever, or any other contagious diſeaſe, in Liverpool. At that time the fever broke out in the Infirmary, deſcribed in page the ſixth of this volume, and two wards being fitted up for the patients infected, one for each ſex, theſe were afterwards occupied by ſuch caſes of fever as occaſionally preſented themſelves. The reception of fever into theſe wards continued for five years and one month, during which time 232 caſes of fever paſſed through them. The wards were ſmall, and under ground; in every reſpect inconveniently ſituated. They held at moſt eight beds each, but were properly adapted to ſix beds only. But they admitted of cleanlineſs and ventilation, and it was there that an opportunity was afforded of employing the affuſion of cold water; of remarking its effects; and of eſtabliſhing the rules that ought to regulate its application.

From the firſt however, the admiſſion of fever into the Infirmary, (which was contrary to one of the original rules of the inſtitution) was objected to by ſome of the friends of that charity. They allowed the benefit ariſing from a receptacle for fever, but they contended that the Infirmary was not adapted to that purpoſe; and that the attempt to render it a hoſpital for fever, the wards already mentioned being all that could be allotted to that diſeaſe, did not afford the relief required, while it obſtructed the original purpoſes of the inſtitution. The want of an aſylum for this diſeaſe elſewhere, overcame theſe objections. But about the beginning of 1793, a contagious fever having prevailed in the work-houſe, two ſpacious wards were fitted up in that building by the Pariſh Committee, for contagious diſeaſes; and an arrangement having been made for the reception of ſuch caſes of fever as preſented themſelves at the Infirmary into the wards of the work-houſe, from that time the admiſſion of fever into the Infirmary has ceaſed.

The wards for fever at the work-houſe, have ſecured that immenſe hoſpital, often containing 1200 perſons, from the ſpreading of fever, to which, previous to their eſtabliſhment, it was perpetually liable; and though they are in the very centre of the building, and cannot be entered but through the common ſtair-caſe, yet no ſingle inſtance has occurred of the contagion extending from them into the other parts of the houſe. They have alſo relieved the Infirmary from the neceſſity of admitting thoſe miſerable wretches, who, under the influence of fever (perhaps caught on ſhip-board) and refuſed admittance into private lodging, were brought up to that hoſpital, where, if not received, they might have periſhed in the ſtreets. But they have not been of ſufficient ſize to anſwer the purpoſe of a general Houſe of Reception for the poor labouring under fever throughout the town, nor has their power of accommodation been employed to its full extent with this view.* The phyſicians of the Diſpenſary who attend the work-houſe, have authority to remove patients on the firſt appearance of fever, from any part of the workhouſe itſelf, into theſe wards; but they have not the ſame authority to remove them from the town. The delay occaſioned by the forms to be paſſed through in obtaining the admiſſion of theſe patients, has very generally prevented the attempt being made, and ſometimes where it has been made and ſucceeded, has occaſioned the relief to come too late.

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* Theſe fever wards contain eighteen beds very conveniently, and from the 20th of Auguſt 1793 to the 31ſt of Auguſt 1797, a ſpace of four years, only 530 caſes have paſſed through them. They have not therefore on an average contained more than ſix fever-caſes at a time. Of theſe 530 caſes, 51 have died, a ſmall proportion, conſidering that 16 died in 24 hours after admiſſion, and were in *articulo mortis* when received.

In conſequence of conſiderations of this kind, a motion was made at the annual veſtry in March 1796, that a ſet of fever-wards ſhould be conſtructed at the expenſe of the pariſh, for the general uſe of the poor, on the plan of the Mancheſter Houſe of Recovery. Two wards, each capable of containing twenty beds, were ſtated to be ſufficient for the town; it was propoſed that they ſhould be erected on the elevated and open ground contiguous to the work-houſe, and be ſupported out of the pariſh rate. And it was aſſerted that this would be a plan, not merely of charity, but of œconomy. The meaſure of eſtabliſhing fever-wards was unanimouſly agreed to, and a committee appointed to carry it into effect; but differences of opinion having ariſen as to the execution of the plan, all proceedings on the ſubject have been ſuſpended. It is earneſtly to be hoped that they may ſpeedily be reſumed.

Contagious diſeaſes, and more particularly fevers, have in general been excluded from the hoſpitals of England; but the evidence of our own Infirmary and work-houſe, of the Cheſter Infirmary, and of various ſimilar inſtitutions, proves, that under proper regulations they may be admitted under the ſame roof with other diſeaſes, without any danger of the infection ſpreading through the building. It is not however to be denied, that the attendance and the arrangement requiſite for patients under fever, render it a matter of convenience and propriety that they ſhould be received into a ſeparate building; and it is a point capable of demonſtration, that hoſpitals for ſuch diſeaſes ſtand pre-eminent in point of utility over all other hoſpitals, thoſe for accidents that require the immediate aſſiſtance of ſurgery, alone perhaps excepted. The benefit derived from hoſpitals in other caſes conſiſts in removing diſeaſe, and is confined to the patient himſelf; but in caſes of contagion, the evils prevented are much greater than thoſe remedied, and the benefit is by this means extended from the patient himſelf to the circle by which he is ſurrounded. The eſtabliſhment of ſuch hoſpitals was firſt ſuggeſted by tracing the contagion which propagates the contagious fever, to its origin, and aſcertaining the power of ventilation and cleanlineſs in preventing and in alleviating the diſeaſe. The arguments for ſuch hoſpitals are ſtrengthened by the diſcovery of the chemical methods of deſtroying this contagion; and if I do not greatly deceive myſelf, they are ſtill farther ſtrengthened by the ſucceſs of that mode of practice in fever, which it is the chief object of this publication to explain and to eſtabliſh. A vigilant exerciſe of all the means of prevention and of cure, might indeed in a ſhort period, ſuperſede the uſe of hoſpitals for fever, by extinguiſhing the diſeaſe; a proſpect in which the philanthropiſt might indulge with more ſafety, if he could calculate with equal confidence on the wiſdom, as on the power of his ſpecies.

CHAP. XVII.

Is there an Inhalation by the Skin?—Caſe of P. M. *Eſq. who died of Inanition.—General Reflections.—Concluſion.*

IN ſpeaking of the origin of animal heat, and of the nature and office of perſpiration *(p.* 175, 179*)* it was queſtioned whether the inhalation of vapour or water through the ſkin into the circulating fluids be eſtabliſhed by a ſufficient induction of facts, and if it occur at all, it was ſuppoſed that the quantity had been exaggerated. Among the ancients however, the doctrine in queſtion was univerſally received, and it has been admitted by the moſt celebrated phyſiologiſts, not merely of the paſt, but of the preſent times. The facts and arguments in ſupport of it are detailed by Haller in the 5th volume of the *Elementa Phyſiologiæ*; but on examination they will be found to afford no direct proof of its truth.* Of the great variety of authors quoted on this occaſion, no one affirms the actual increaſe of the weight of the body in the warm bath, Deſſault excepted, and this is a point that Haller himſelf never ſeems to have ſubjected to experiment.

The inhalation through the ſurface in vegetables, in inſects, and in certain of the amphybia, are the analogies that give the moſt direct ſupport to this doctrine, applied to the human ſpecies. It is however controverted by many facts, and if it ſhould ultimately be eſtabliſhed, it muſt be reared on a new foundation. When I was at Buxton in 1788, I made an experiment on the effects of bathing on the weight of my body, and after half an hour's immerſion, I found it rather diminiſhed than increaſed. This experiment had I believe been made

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* The arguments drawn from the appearances that occur in the dead body, may be wholly ſet aſide; as may the inſtances of certain narcotic vegetables, applied to the ſkin, (e. g. tobacco and white hellebore) occaſioning ſickneſs at the ſtomach, which are caſes of ſympathy, or of obſorption *after the cuticle had been eroded.* The arguments drawn from the increaſe of weight in moiſt air, and from the vaſt diſcharge of urine in diabetic caſes, are alſo wholly fallacious. In diabetes we now know there is no abſorption by the ſkin, and the phenomenon in queſtion, which has been much exaggerated, muſt be accounted for by the action, not of the ſkin, but of the lungs.

before by Dr. Pearſon, with the ſame reſult; it has ſince been repeated frequently with great care,* and it is aſcertained as a fact, that no increaſe in the weight of the body is produced by immerſion in water of the heat of 82°. In the year 1790, I had a patient in diabetes, whoſe cuticle, as is uſual in that diſeaſe, was in a morbid ſtate; and being deſirous of trying how far the inordinate action of the kidneys might be affected by a gentle ſtimulus applied to the ſkin, I immerſed him in the bath of the temperature of 96°, weighing him before and after immerſion. There was no variation in the weight. This experiment has lately been made by Dr. Gerard, in another caſe of Diabetes, an account of which is given in the publication of Dr. Rollo;* and as it was repeated a great number of times with the utmoſt care and accuracy, it may be conſidered as eſtabliſhed that immerſion in the warm bath in Diabetes, produces no increaſe of weight. I have made five different experiments of the ſame kind on myſelf, varying the heat of the bath from 87°. to 95°. but never in any inſtance found my weight augmented. It may be ſaid however, (and it has been ſaid) that though in Diabetes, where the epidermis is diſeaſed, no liquid is inhaled; and though in health, where the veſſels are full, no abſorption may take place; yet that where the body is waſting from a want of the proper food through the ſtomach, the plaſtic powers of nature may be exerted to ſupply the defect, and to excite an inhalation through thoſe pores on the ſurface, by which exhalation is uſually performed.

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* In one caſe, a ſum of money depended on the iſſue, and a friend of mine ſubmitted to be the ſubject of the experiment.

* *Rollo on Diabetes Mellitus, v.* ii. *p.* 72.

The following caſe will throw ſome light on this point. I give it however not merely with this view, but as containing other particulars that appear to me curious and intereſting.

In Auguſt 1795, a gentleman of ——ſhire, aged 66, applied for my aſſiſtance in a dyſphagia, (obſtructed deglutition) with which he had been for ten or twelve months afflicted. At firſt the complaint was ſlight, and occurred only when he attempted to ſwallow dry and hard ſubſtances; it afterwards extended to ſolids of every kind; and at the time I firſt ſaw him, though ſtill able to paſs down liquids, the quantity he could ſwallow was not ſufficient for his nutrition, and he was conſiderably reduced. On the introduction of the bougee into the gullet, it paſſed about two inches eaſily, but then met with an obſtruction, which by a moderate preſſure was overcome. It then paſſed eaſily ſeven or eight inches more, but experienced a firm a firm reſiſtance in the lower part of the tube, towards its termination in the cardia, which the ſkill and patience of Mr Park could not ſurmount, and which finally baffled every reſource of his art. Though we had not an opportunity of examining the Oeſophagus after death, yet the nature of this obſtruction was evident. It doubtleſs proceeded from a ſchirrous tumour, gradually increaſing, which at firſt diminiſhed the paſſage, and in the end obſtructed it wholly; and it exiſted in that part of the tube, which, from its ſuddenly narrowing, is, as Dr. Hunter has remarked, moſt expoſed to the cauſes, and moſt frequently the ſeat of this diſeaſe. Mr. M. never had any conſiderable pain from the preſſure of the bougee on the tumour, and though he expectorated (if the expreſſion may be uſed) much mucus, there never was any purulence diſcharged, or any reaſon to believe that the tumour had ulcerated.

The laſt time Mr. M. ſwallowed in any quantity, was on the 17th of October. On the evening of that day a ſudden increaſe of the obſtruction came on, and from this time forth he was able to ſwallow only a table ſpoonful of liquid at a time, and with long intervals. It was with difficulty that he got down ſeven or eight ſpoonfuls of ſtrong ſoup in the day, and this quantity gradually diminiſhed.

On the firſt of November, the paſſage ſeemed wholly obſtructed. As he waſted rapidly, we were obliged, from the 18th of October to employ the ſubſidiary modes of nutrition, and he went into the tepid milk and water bath, and had a nutritive clyſter adminiſtered once a day.

As Mr. M. himſelf was far from deſpairing of his recovery to the laſt, the affectionate friends around him did not wiſh him to be informed of his real ſituation. He continued therefore to ſwallow two or three table ſpoonfuls of milk daily, till about the 15th of November, on the ſuppoſition that ſome might paſs; but though it reſted in the Oeſophagus ſome time, it was conſtantly ejected at laſt; and being fatigued by this expectoration, which diſturbed his reſt, and by which he perceived that the whole of the milk was thrown up, for the laſt twenty days of his life he made no attempt to ſwallow, but contented himſelf with waſhing his mouth and fauces with wine and water.

Mr. M.'s friends, though they could not but foreſee the iſſue of his caſe after all attempts to remove the obſtruction had failed, yet deſired that his life might be prolonged to the uttermoſt. The following plan was therefore adopted with this view, on the laſt days of October, and it was perſevered in with the variations afterwards to be mentioned,

tioned, till the commencement of the delirium that preceded his death.

Every morning at eight o'clock, he had a clyſter, conſiſting of eight ounces of ſtrong broth, made chiefly of membranous parts of beef (being the moſt nutritious, according to Dr. Fordyce) into which was rubbed two yolks of an egg, and to which was added forty drops of laudanum. This was repeated at three in the afternoon, and again at nine in the morning; but in the evening, inſtead of forty, one hundred and twenty drops of laudanum were added, to compenſate the want of an anodyne draught, to which he had been accuſtomed at the hour of reſt. Previouſly to this however, he was placed up to the neck in a tepid bath, of which one fourth was milk, and the reſt water; the whole quantity amounting to twenty-four gallons. The temperature was fixed at 96°. to accommodate his ſenſations, and the time of immerſion was gradually prolonged from forty-five minutes to an hour.

The ſize and repetition of the clyſters were determined by our experience of what the rectum would retain; the laudanum being added to increaſe its retention, as well as to allay irritation, and to ſtimulate the living energy. In a few days the retention of the rectum improved, and enabled us to enlarge the clyſters to 10 oz. of broth and three yolks of eggs each; to which, from the 12th of November, 8 oz. of white wine were added; the laudanum being increaſed to 60 drops for each of the clyſters in the day, and to 150, 180, 200, and 250 drops for the evening clyſter. Thus the whole of his nutriment for twenty-four hours conſiſted of 30 oz. of broth, 24 oz. of wine, 9 yolks of eggs, and from 250 to 380 drops of laudanum, by clyſter; with what liquid might be ſuppoſed to be taken up by the abſorbents of the ſurface in the bath. Mr. M. was very tall, and naturally corpulent. In health he had weighed 240lbs. But imperfect nutrition had gradually reduced him, and at the time of commencing this proceſs, he only weighed 179lbs. On the 20th of November he was reduced to 154lbs.; on the 24th to 149lbs. This was the laſt time we had an opportunity of weighing him. He ſeems to have waſted about 5lbs. in four days, and on the ſecond of December, when his dilirium commenced, he probably weighed 138 or 139lbs. having loſt upwards of 100lbs. of his original weight. He lived till the 6th of December, but from the 2d, the rectum no longer retaining the clyſters, they were of courſe omitted, as well as the bath.

About the 25th and 26th of October, there was reaſon to ſuppoſe that Mr. M. would have had a ſhorter date. He had ſwallowed a very ſmall portion of nutriment for the preceding eight days, and the plan of nutrition juſt mentioned, though adopted in part, was not fully eſtabliſhed. His tongue and lips were at this time become dry; a bliſter applied to the ſternum, diſcharged a thick coagulable lymph only; and his urine, reduced to a few ounces in the twenty-four hours, was become extremely high coloured, and almoſt intolerably pungent. In a few days however this ſtate of the tongue went off; the ſkin became perfectly ſoft, and was often covered with a very gentle perſpiration; the urine flowed without pain, and of the natural appearance, the quantity being from 24 oz. to 36 oz. in the twenty-four hours—more than the whole of his loſs of weight in the ſame ſpace of time.

Mr. M. had in general a ſtool after every fifth or ſixth clyſter, that is, three or four times a week. The ſubſtance of theſe ſtools was ſolid, and nearly of the natural fœtor, and the maſs perfectly homogenous. The colour was lighter than natural, as is the caſe in obſtructions of the bile duct; their whole appearance reſembling what we find in caſes of diſſection in the termination of the Ileum or beginning of the Colon, with the exception of being much leſs liquid. The concoction of the Fœces, if I may ſo ſpeak, was much more perfect towards the end, than at the beginning of this period; at firſt they looked curdy, but at laſt they approached to the natural appearance.

The heat, examined by the thermometer, was natural, and nearly uniform from firſt to laſt. After the clyſters there was generally ſome fluſhing of the cheek, and drowſineſs, with ſome increaſe in the warmth of the extremities; but under the axilla there was ſeldom any variation, and when it roſe there a ſingle degree, it was ſpeedily lowered by a gentle diaphoreſis.

The pulſe, during the period of the ſcarcity and pungency of the urine, was diſturbed, and more frequent. It afterwards ſubſided to the natural ſtate, and during the whole of the month of November, was not to be diſtinguiſhed from that of a perſon in health. On the morning of the firſt of of December it became ſmall and frequent; and ſtill more frequent, though ſtronger, after the delirium commenced; the ſtate of the pulſe, as well as almoſt all the other ſymptoms, from that time forth, very nearly reſembling the ſymptoms of the laſt ſtage of fever, when it terminates fatally. During this delirium, a perpetual and indiſtinct muttering occurred, with great reſtleſsneſs and agitation; the ſurface and extremities were ſometimes

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of a burning heat, ſometimes clammy and cold. The eyes loſt their common direction, the axis of each being turned towards the noſe. In this ſtate however the ſenſibility of the retina was not impaired, but rather encreaſed, for he ſcreamed out on the light of the window being admitted, to which before he had been accuſtomed. At this time alſo the ſenſe of touch ſeemed more than uſually acute, for he appeared diſturbed with every accidental breath of air. The dilirium, and the derangement of viſion commenced nearly together, but we obſerved the derangement of viſion firſt. On the firſt of December he complained that he ſaw objects occaſionally double; but it was not till the ſucceeding day, that any conſiderable incoherence of mind was obſerved. The pulſe became feeble and irregular on the fourth; the reſpiration, which had been ſingularly undiſturbed, became laborious; the extremities grew cold; and in ninety-ſix hours after all means of nutrition, as well as all medicine had been abandoned, he ceaſed to breathe.

Mr. M. complained very little of hunger. Occaſionally he expreſſed a wiſh that he could ſwallow, but not often, nor anxiouſly; and when queſtioned on the ſubject of his appetite, always declared that he had no hunger to occaſion any uneaſineſs. The clyſters evidently relieved the ſenſe of hunger, and the opium they contained, ſeemed to have a principal ſhare in producing this relief. It occaſioned quiet and reſt after each clyſter, and allayed every kind of deſire or appetite.

Neither was Mr. M. much diſturbed with thirſt. This ſenſation was indeed troubleſome during the firſt days of his abſtinence; but it abated, and as he declared, was always removed by the tepid bath, in which he had the moſt grateful ſenſations.

Mr. M.'s ſpirits were uncommonly even, and his intellect perfectly ſound. He occupied himſelf a good deal in his private concerns, and as uſual intereſted himſelf in public affairs. As we diſcouraged all unneceſſary exertion, he ſpent a great part of his time in bed; but till the laſt few days of his life, he dreſſed and undreſſed himſelf daily, and walked, not only about his room, but through the houſe. His nights were quiet, and his ſleep ſound, and apparently refreſhing. Towards the end of this period, he ſaid he had very lively dreams, but they were all of a pleaſant nature. The laſt converſation I had with him was on the morning of the ſecond of December. He told me that he had had a very gay evening with two Yorkſhire Baronets, whom he named; they had puſhed the bottle about freely; many jokes had paſſed, at the recollection of which he laughed heartily, a thing uncommon with him. When I obſerved that this was a very lively dream, he ſeemed to endeavour to recollect himſelf, and for a moment appeared ſenſible that the ſcene had paſſed in ſleep. But recurring to the circumſtances, he ſpoke of them again as realities, and I made no further attempt to undeceive him. He ſaid he had been walking out that morning, and was pleaſed to ſee the ſpring ſo far advanced—he had never felt the air ſo refreſhing. At this time, though his pulſe was become rapid, his reſpiration was natural, and his ſpeech diſtinct, and without effort. He was extremely weak, and ſcarcely able to turn himſelf in bed, to which he had for ſeveral days been entirely confined. No man had ever perhaps approached death by ſteps more eaſy, and it was earneſtly to be wiſhed that his remaining moments might be few. This wiſh was not gratified. In about an hour afterwards his dilirium became complete, during which his ſtrength evidently increaſed, and the ſtruggle commenced which has already been deſcribed, and which laſted ninety-ſix hours. He died in his ſixty-ſeventh year.

I have been the more particular in noting this caſe, becauſe I have not found the methods of ſupporting life under impaired or obſtructed deglutition, ſufficiently detailed, nor the death from inanition deſcribed with ſufficient accuracy. A knowledge of the firſt may on many occaſions be important; and an acquaintance with the laſt may poſſibly throw light on ſome difficult queſtions that reſpect, not merely the vital, but the intellectual phenomena.

I have not found any caſe deſerving of credit, in which life was ſo long preſerved without the aid of the ſtomach, two excepted, one recorded by Ramazzini,* and the other mentioned by Mr. Cruikſhanks.‡ The caſe deſcribed by Ramazzini was that of a nun; and the obſtruction was not mechanical, but ſpaſmodic; or as he himſelf ſuppoſes, ariſing from a paralyſis of the Oeſophagus. She is ſaid to have lived ſixty-ſix days without ſwallowing, the whole of her nutriment conſiſting of a ſingle clyſter of broth, and two yolks of eggs, adminiſtered daily. The dyſphagia, which came on ſuddenly, ſeems alſo to have gone ſuddenly off, and ſhe finally recovered. The caſe is not given with ſuch minuteneſs by Ramazzini as to inform us whether any attempts at ſwallowing were made during the dyſphagia. As the obſtruction was not mechanical, it is probable that ſuch attempts were made daily; and if this be ſuppoſed, a ſuſpicion may be entertained from the nature of the affection, that more or leſs nutriment might be ſwallowed. It is obſerved by Ramazzini, that the ſufferings of his patient were ſo great on the 30th day, that ſhe was anxious for death, and refuſed to admit the clyſter, which however was adminiſtered during Syncope, and with immediate and happy effects. In this reſpect her ſituation was perfectly contraſted by that of Mr. M. who enjoyed eaſe and ſerenity of mind till the laſt final ſtruggle, as has already been mentioned.

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* *Ramanzzini Opera Omnica Genevæ*, 1717, *p.* 173.

‡ *The Anatomy of the Abſorbing Veſſels, London*, 1786, *p.* 101.

The caſe of Mr. Cruikſhanks is not given at length, but alluded to in the following words. "That the ſurface of the ſkin abſorbs other fluids "which come in contact with it, I have not the "leaſt doubt. A patient of mine with a ſtricture "of the Oeſophagus, received nothing, either ſolid "or liquid, into the ſtomach *for two months*; he "was exceedingly thirſty, and complained of "making no water. I ordered him the warm bath "for an hour evening and morning *for a month*; "his thirſt vaniſhed, and he made water in the ſame "manner as when he uſed to drink by the mouth, "and the fluid deſcended readily into the ſtomach." It is to be regretted that a caſe ſo curious is not given in detail. It does not appear what other means of ſuſtenance Mr. Cruikſhanks employed beſides the tepid bath, eſpecially during the month of abſtinence in which the bath itſelf does not ſeem to have been uſed. On ſome future occaſion it is to be wiſhed that he may ſupply theſe deficiences; eſpecially as the information I am about to give, will ſhew that the concluſion he draws from this caſe, of the abſorption of the ſkin, is fallacious.

Notwithſtanding what I had obſerved formerly, I fully expected in the caſe of Mr. M. an increaſe of weight from the immerſion, becauſe he always expreſſed great comfort from the bath, with abatement of thirſt, (which however was never great) and becauſe ſubſequent to the daily uſe of it, the urine had flowed more plentifully and become leſs pungent. I expected it alſo from the authority of general opinion, and was curious to know the extent of the abſorption. I weighed him three different times. The firſt time he was weighed with his cloathes on, before he went into the bath; and naked when he returned from it. Our proceſs was therefore complicated. He appeared to have gained 30 oz. in weight, which gave him much comfort; and this circumſtance prevented me from undeceiving him when I afterwards diſcovered our error.* The ſecond time he ſtepped perfectly naked upon

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* I diſcovered that we had been incorrect before the ſecond experiment,

Merlin's balance immediately before immerſion, and again immediately after it, his body being previouſly dried. The weights were never moved. The reſult ſurpriſed me: I could not diſtinguiſh the ſlighteſt variation in the weight of his body, though my beam would have detected a ſingle drachm. I repeated this experiment four days afterwards, and with a ſimilar reſult—On both of theſe occaſions the immerſion continued an hour, and a conſtant friction had been kept up nearly the whole of this time on the inner ſurface of the thighs, with the view of increaſing the action of the abſorbents.

It is worthy of obſervation that there was neither increaſe nor decreaſe of weight in the bath; yet Mr. M. was at this time waſting twenty ounces in twenty-four hours, and conſequently if there was no abſorption in the bath, it might be ſuppoſed that he would have loſt the 56th of an ounce during the hour of immerſion. It is the more remarkable that there was no decreaſe, becauſe in the bath the ſkin always ſoftened, and the forehead became covered with a gentle moiſture, which probably aroſe from the evaporation of the liquid in the bath. At the time however I conſidered this as the matter of perſpiration, becauſe after leaving the bath, and going into a bed previouſly warmed, he uſually fell into a gentle and general diaphoreſis, the ſoother of every irritation, and the harbinger of refreſhing ſleep.

So far the caſe is tolerably complete. I lament that I cannot give the appearances on diſſection, as we were not permitted to examine the body after death.

The reflections ſuggeſted by this caſe are numerous: I ſhall preſent one or two that ſeem important.

The natural ſtate of the pulſe during the firſt month of Mr. M.'s abſtinence, when his ſtrength was daily decaying, demonſtrates, that the motion of the heart and arteries gives no certain indication of the ſtrength of the ſyſtem. Pure debility, within certain limits, does not ſeem to produce a frequent pulſe, nor in all caſes, a feeble one; but when carried to a certain degree, it excites that commotion of the ſyſtem, in which—firſt the contractions of the heart, and afterwards of the lungs, become more rapid; which in popular language may be denominated the laſt ſtruggle of nature, and which may be conſidered as indicating the approach of death. To endeavour to reconcile theſe phenomena to any of the prevailing theories of vital motion, would be a fruitleſs attempt. Death has not an uniform aſpect, but its *phaſes* do not appear to me numerous. The concluding ſcene in the life of Mr. M. may illuſtrate the form of its approach, as well as the nature of the ſymptoms in the laſt ſtage of fever, and of various other diſeaſes, in which ſimilar ſymptoms occur.

The natural ſtate of the heat, during the inaction of the ſtomach, might prove, if it were neceſſary, how little the vital heat is connected with the function of digeſtion, and renders it probable, that the general increaſe of heat which follows the application of many ſtimulating bodies to this organ, is not the direct effect of the action of the ſtomach itſelf, but of its influence on the heart, or diaphragm, or both. It is aſſerted by Haller, that in caſes ſimilar to that of Mr. M. the heat has been found diminiſhed. The obſervations made by the thermometer in this caſe, throw a doubt on the aſſertion; while the great facility with which Mr. M. parted with his heat, concurs with a variety of correſponding facts to ſupport the poſition, that in thoſe ſituations, where the generation of heat is natural, the power of retaining it in the ſyſtem is in the ratio of the force of the living principle.

But what ſhall we conclude reſpecting the abſorption of water by the ſurface of the body in the warm bath?—Admitting that it did not take place in the caſe of Mr. M. it may be ſuppoſed this did not ariſe from the impermeability of the cuticle, but from the heat of the bath being too high. It may be added, that the analogy of the vegetable kingdom ſupports this ſuppoſition, ſince vegetables are found to exhale in warm and dry air during the day, but to abſorb largely under the moiſture and chillneſs of night. Conſiderations of this kind might probably influence ſome learned phyſicians in London who were conſulted in this caſe, to propoſe that the heat of the bath ſhould be as low as 80°.—It was however raiſed to 96° for two reaſons: the firſt, that I had found no increaſe of weight in the Buxton bath at 82°; the ſecond, that as Mr. M. parted with his heat eaſily, he could not ſupport immerſion with comfort in a lower temperature.

It is however more likely to be ſuppoſed, that though there was no increaſe of weight in the caſe of Mr. M., there might be an abſorption in the bath which was countervailed by the increaſed exhalation.—The experiments I have detailed do not enable us to decide this point abſolutely; but from them, and from various other conſiderations, I entertain little doubt, that though the exhalents of the ſkin pierce the epidermis, and come into contact with the external air, the mouths of the abſorbents terminate under it, and are covered by it; and that while it remains ſound and entire, no abſorption of ſolid, liquid, or aeriform elaſtic fluid, takes place on the ſurface. In the inſtances that are ſuppoſed to favour the contrary opinion, it will be found, that the article abſorbed is forced through the epidermis by mechanical preſſure; or that the epidermis has been previouſly deſtroyed by injury or diſeaſe; or if ſound, that the article applied to it is of an acrid nature, which firſt erodes this tegument, and then coming into contact with the mouths of the lymphatics under it, is of courſe abſorbed.*

At the inſtant that theſe obſervations are committed to the preſs, my attention has been pointed to the valuable memoir on this ſubject, read before the Academy of Sciences by M. Séguin, of which ſome account is given by Fourcroy in the third volume of "*La Médicine eclairée par les Sciences phyſiques, p.* 232." The concluſions of M. Séguin, correſpond in every particular with thoſe which I have already laid down, and being founded on a great variety of experiments, made for the expreſs purpoſe of determining on the cutaneous abſorption, they appear to me to give his inferences all the ſolidity of truth. The objection, that though there is no increaſe of weight in the bath, there may be an abſorption, which is countervailed by the exhalation, M. Séguin found a ſatisfactory mode of invalidating. He diſſolved in the water of the bath, in which he made his experiments, ſubſtances which produce a ſpecific effect when received into the ſyſtem, by which their inhalation might be aſcertained. He employed the oxygenated muriat of mercury in ſolution, on a number of venereal patients, and while the epidermis was entire, he never perceived a ſingle inſtance of ſalivation, or even of amendment of their complaints. But in caſes where a conſiderable portion of epidermis was injured or deſtroyed, as in the itch, the ſpecific effects of mercury on the ſyſtem were produced. The experiments of M. Séguin are very numerous, and appear to be deviſed ſo as to meet every objection. He concludes from the whole, that while the epidermis is entire, the abſorbents of the ſurface neither take up water nor air, nor any ſubſtance diffuſed or diſſolved in either.

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* The abſorption of mercury, ſulphur, oil of turpentine, and ſimilar ſubſtances, rubbed on the ſkin in the form of unguents, comes under the firſt of theſe deſcriptions; the abſorption of the ſame ſubſtances, and in ſome rare inſtances of lead from the ſurface of wounds, and of herpetic or other eruptions, under the ſecond; the abſorption of cantharides, aloes, &c. under the third. I am not ignorant of the experiments of Dr. Falkner and Mr. Albernethy, nor wanting in due reſpect for thoſe gentlemen; but my own obſervations, ſupported by the experiments of M. Séguin, induce me to reject their concluſions. A probable ſource of error in the experiments ſuppoſed to afford a direct proof of cutaneous abſorption, is, that they were made on a ſmall part of the body, e. g. the arm or leg, and not on the whole; though if the mouth and noſtrils were left free for reſpiration, it would be as eaſy, and much more correct, to make experiments for this purpoſe, as well as for determining the quantity of exhalation, on the whole of the ſurface, as on a part. Dr. Marcard, who wrote expreſsly on the effects of the bath, has adopted the theory of abſorption through the ſkin, without making any experiments,

Admitting that the the inferences of the friend and coadjutor of Lavoiſier are juſt, how are we to explain the refreſhment and abatement of thirſt, experienced by Mr. M. in the bath. Referring to what is ſaid in pages 162, 163, on the nature of thirſt, we may here ſhortly obſerve, that as liquids thrown into the ſtomach relax the exhalents of the ſurface, ſo doubtleſs this action is reciprocal; and the exhalents of the ſurface being relaxed in the warm bath, a relaxation in thoſe of the ſtomach enſues, by which the ſenſe of thirſt is abated. In the cold, as well as in the hot ſtate of intermittent fever, the thirſt is great; but it is always leſſened, and generally removed, on the acceſſion of the ſweating ſtage, though no liquid ſhould have been ſwallowed.

Will this explanation, it may be aſked, account for the benefit derived in accidents at ſea, when no drink can be produced, from immerſing the body in water, or applying wet ſubſtances to its ſurface? It will explain it in part perhaps, but not wholly.—In the torrid zone, where the greater part of theſe inſtances have occurred, the immerſion of the body in water, or the application of wet clothes to the ſkin, will keep the ſurface cool, by defending it againſt the direct rays of the ſun, and thus prevent the debility produced by an accelerated circulation; it will alſo defend it, in whatever climate, from the waſting effect of evaporation, and thus be a means of prolonging the ſtrength. This reaſoning is ſupported by a fact which may be inferred from the circumſtances I have mentioned, and which is eſtabliſhed by the experiments of M. Séguin; that the body waſtes much leſs in water than in air.

In the Buxton bath, I found my weight very ſlightly diminiſhed, but in the caſe of Mr. M. and in all our experiments on diabetic patients, there was neither diminition nor increaſe in the bath. In the experiments of M. Séguin, there was always ſome diminition in the bath of whatever temperature, which he aſcribes to the pulmonary perſpiration. How is this difference accounted for? Though the cutaneous exhalation may have been ſtopped in Mr. M. during immerſion, yet allowing the perſpiration by the lungs to have gone on, this alone, ſuppoſing no abſorption, ought to have produced a diminution of weight in the bath. To this it may be replied, that though no abſorption through the ſkin takes place in the bath, that by the lungs goes on, while in the heat of 96°. the evaporation from the ſurface of the water, by loading with moiſture the air inhaled, prevents its taking up the moiſture in the lungs, and thus diminiſhes the pulmonary perſpiration.—Againſt this ſource of error it is probable that M. Séguin guarded.* It is in this way that the increaſe of the weight of the body in moiſt air, which is ſaid ſometimes to occur, and which has been ſuppoſed to prove a cutaneous abſorption, may be accounted for. In moiſt air the abſorption of the lungs from the atmoſphere goes on unchanged, while the pulmonary, as well as the cutaneous perſpiration, is diminiſhed, and thus the increaſed weight of the body may be explained. On this point however more accurate experiments are required.

It was obſerved, that the diſcharge by urine alone, in the caſe of Mr. M. exceeded much in weight the waſte of his whole body; and it cannot be doubted that the diſcharge by ſtool and perſpiration, exceeded the weight of the clyſters. Thus it appears that the egeſta exceeded the ingeſta, in a proportion much greater than the waſte of his body will explain. How, it may be ſaid, is this accounted for, unleſs by cutaneous abſorption? In the ſame way, it may be anſwered, by the action of the lungs.

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* I ſay probable, becauſe I have not ſeen the memoir of Séguin himſelf, but only the abſtract of it given by Fourcroy, who does not enter into the details.—An account of this abſtract is given very accurately, and pretty much in detail in the Monthly Review Enlarged, vol. xii. p. 514.

That the oxygen inhaled in reſpiration from the atmoſphere unites in part with the carbon of the blood, to form carbonic acid gas, and in part with the hydrogen, to form water, is generally admitted. In inſtances of inanition we may ſuppoſe the proportion in which it unites to theſe different ſubſtances to be varied by the plaſtic power of life; that the formation of water is greater than uſual, and that of carbonic acid gas leſs. Thus a portion of the oxygen, which, in the ordinary courſe of things *is exhaled in the form of an elaſtic vapour, being abſorbed in the form of water*, will ſerve to retard the waſte of the body, and to furniſh the blood with the proportion of lymph, which affords a proper ſupply to the ſecretions of urine and perſpiration. In the caſe of Mr. M. there was no appearance of that acrimony of the fluids which might naturally have been expected from the ceſſation of the ſupply of chyle, and which Haller deſcribes as the conſtant effect of inanition.

This theory of the increaſed formation of water in the lungs, has been employed by Dr. Rutherford of Edinburgh, to account for the vaſt diſcharge by the bladder in diabetes. It ſeems alſo to account for the phenomenon of the egeſta exceeding the ingeſta, ſo often obſerved in that diſeaſe. It illuſtrates the doctrine of Séguin, in the memoir already quoted, of which Fourcroy gives an account in the following words—*Le diabète ne provient point de l'eau abſorbée dans l'air par la peau, mais du reflux de celle qui ne peut pas être enlevée aux poumons par l'air trop chargé d'humidité*—a doctrine which appears to be defective, from not pointing out the ſource of the increaſed quantity of water in the lungs. Beautiful as the theory of Dr. Rutherford is, it cannot be conſidered as eſtabliſhed in diabetes, or in inanition (to which I have extended it) till the air expired by patients under theſe diſeaſes ſhall be examined. In the mean time I may obſerve, that the application of it to the caſe of Mr. M., accords with the uſual œconomy of the living principle, which in remedying the defect of particular organs, does not transfer their powers entire to other parts of the ſyſtem, but modifies or increaſes the natural action of ſome other organ, by which that deficiency may be to a certain degree countervailed. Such an exertion of the living principle was evident in the caſe of Mr. M., in the increaſed powers of the rectum, which have already been deſcribed. Theſe vicarious actions of the ſyſtem are however defective, whether ariſing ſpontaneouſly, or by the aſſiſtance of art. No applications of food to the inner ſurface of the rectum can long ſupply the abſence of food in the ſtomach, nor can the want of liquid in that organ be long countervailed by applications of moiſture to the ſurface, aided by the modified action of the lungs. After a certain time, though immerſed in water to the chin, the thirſt will prove intolerable without drink, as the ſhipwreck deſcribed in the Appendix will teſtify *(Appendix, p.* 24*)* and the ſufferings of Tantalus, which the theories of Dr. Marcard led him to deride, will be rated as highly by the naturaliſt as by the poet.*

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* Dr. Marcard, in the work quoted, *p.* 135, after aſſerting the great inhalation of water by the ſkin in the bath, declares, that no naturaliſt will eſtimate the ſufferings of Tantalus highly. It ſeems probable, that immerſion in tepid water is more effectual in abating thirſt, than immerſion in cold water.

The account I have given of Dr. Rutherford's theory of diabetes, I derive from the valuable inaugural diſſertation of Dr. Marcet, of Geneva, juſt publiſhed, preſented to me by the ingenious author. To Dr. Marcet I am alſo indebted for pointing out the memoir of M. Séguin, whoſe doctrine he himſelf has controverted, from giving, I apprehend, more weight to the authority of Haller, than on inquiry it will be found to deſerve on this ſubject.

The explanation offered by Dr. Marcet of the increaſed heat in diabetes, will be found I believe irrelevant.—In three caſes of this diſeaſe which I have examined, the heat previous to the medical treatment was leſs, not greater than natural, notwithſtanding the almoſt unanimous concurrence of authors in the contrary ſtatement; a proof among many others of the inaccuracy of the accounts we have of temperature in diſeaſe.

If the non-abſorption of the ſurface of the body be eſtabliſhed; it will aſcertain, that contagion is received into the ſyſtem, in the ordinary courſe of things, by the lungs only; and perhaps juſtify a practice common, as I am informed, among our more experienced ſeamen on the coaſt of Guinea, and other warm climates, who when expoſed during the night to a breeze from the marſhes, wrap their heads in a ſea-cloak, or other covering, and ſleep fearleſsly on the deck with the reſt of their bodies nearly naked, from the heat.

It is with pleaſure I lay down my pen, though other obſervations crowd upon me. If what I have already offered ſeem tedious, the intereſt of the ſubject will plead my apology. It is no otherwiſe connected with the reſt of the volume, than as it regards the moſt important functions of the living principle—But I have not been very ſtudious of rigid method in this little volume, which is much prolonged beyond my original intention.—In the preſent ſtate of our ſcience the enlightened reader will thank me for avoiding ſyſtematic arrangement; and recommending myſelf to his candour, I bid him adieu!

THE END.

APPENDIX.

Nº. I.

Of Tetanus, and of Convulſive Diſorders.

Read before the Medical Society of London, May 10, 1790, and extracted from the 3d volume of their Memoirs.—Page 147.

THE various objects which engage your attention will not admit of much of your time being employed on a ſingle paper, and I have undertaken to write on a ſubject of ſome importance. It will be proper for me therefore to come directly to the point, and to avoid as much as poſſible every thing that is ſuperfluous. What I have to offer ſhall be confined to the treatment of convulſive diſorders, and eſpecially of tetanus.

For this diſeaſe, ſo terrible in its progreſs, and ſo generally fatal in its iſſue, ſeveral new remedies have been propoſed within the laſt thirty years, and each in its turn has had ſome ſhare of public confidence. But opium, mercury, the cold bath, and wine, are thoſe only which ſeem entitled to any reputation, and of each, it has been my fortune to have had ſome experience. This experience I proceed to give you with all the clearneſs and faithfulneſs in my power.

In the courſe of nine years' practice I have ſeen ſeven caſes of this diſeaſe; of which two were idiopathic, and five the conſequence of wounds. Of theſe in order.

1. George Gardner, a ſoldier in the Staffordſhire militia, was put under my care by his officers, on the 20th of February, 1781. About a fortnight before, after ſevere dancing and hard drinking at a country wedding, in which he had been employed two days and nights, he fell ſuddenly into a fit, which laſted an hour and a half, during which his conſciouſneſs was aboliſhed. On recovery he was affected with ſlight twitchings, which gradually encreaſed, and were followed by fixed ſpaſmodic contractions in different parts of the body, but more affecting the left ſide than the right. He had, when I ſaw him, all the ſymptoms of tetanus. The head was pulled towards the left ſhoulder, the left corner of the mouth was thrown upwards, the eyes were hollow, the countenance pale and ghaſtly, the face and neck bedewed with a cold ſweat; but his moſt diſtreſſing ſymptom was a violent pain under the enſiform cartilege, with a ſudden interruption of his breathing every fourth or fifth inſpiration, by a convulſive hiccup, accompanied by a violent contraction of the muſcles of the abdomen and lower extremities. He felt on this occaſion as if he had received an unexpected blow on the ſcrobiculus cordis. Before I ſaw him he had been bled and vomited repeatedly, and had uſed the warm bath, not only without alleviation, but with aggravation, of his complaints. The three firſt remedies mentioned were uſed here in ſucceſſion, viz. opium, mercury, and the cold bath.

He firſt took a grain of opium every other hour, afterwards a grain every hour, and at laſt two grains every hour;

but he grew worſe and worſe during the two days on which this courſe was continued. The ſpaſms extended to the the back and ſhoulders, the head was at times retracted, and the muſcles of the abdomen partook of the general affection. Being no longer able to ſwallow the pills, he took no medicine of any kind on the night of the 22d, in the courſe of which general convulſions came on, and returned once or twice in every hour. The tincture of opium (liquid laudanum) was now directed to be given, and an ounce of the quick-ſilver ointment to be rubbed in on each thigh. In twenty-four hours he took two ounces and a half of the tincture without ſleep or alleviation of pain. The doſe being increaſed, in the next twenty-ſix hours he ſwallowed *five ounces and a half* of the laudanum, a quantity which, at that time, was I believe unexampled. He lay now in a ſtate of torpor. The rigidity of the ſpaſms was indeed much leſſened, and the general convulſions nearly gone; but the debility was extreme; a complete hemiplegia had ſupervened; the patient's eyes were fixed, and his ſpeech faultering and unintelligible.

As this young ſoldier appeared on the utmoſt verge of life, it ſeemed no longer ſafe to continue the laudanum, which had relieved ſpaſm only in ſo far as it had brought on general paralyſis. Intermitting this medicine therefore, ſmall doſes of camphor were occaſionally given in a liquid form, but the chief attention was directed to ſupporting the ſtrength by ſuch nouriſhment as could be ſwallowed. Gruel, with a ſmall quantity of wine, was ordered for him, though with much caution; for at that time I was not inſtructed with what ſafety and efficacy this laſt article might have been adminiſtered. For the next ſix days he ſeemed to revive: the general convulſions kept off, though the twitchings and convulſive hiccup continued. But on the night of the firſt of March he was ſeized, during ſleep, with a convulſion as ſevere as ever, and this was followed by a return of all his ſymptoms with their former violence. The jaws were indeed more completely locked than before, deglutition was become impoſſible, and the pain under the enſiform cartilege was ſo extreme as to force from the patient the moſt piercing cries. At this time the effects of the quickſilver ointment were apparent in the fœtor of the breath, and in a conſiderable ſalivation.

Had poor Gardner been a man of any rank, or indeed had he been ſurrounded by his family, it is moſt probable that we muſt now have abandoned him to his fate. But our proceedings being neither obſtructed by the prejudices of ignorance, nor the weakneſs of affection, another, and a laſt effort for his life was reſolved on. Having heard that the cold bath had been employed with ſucceſs in tetanus in the Weſt Indies, particularly by Dr. Wright of Jamaica, and Mr. Cochrane of Nevis, and this practice correſponding with certain ſpeculations of my own, I had recourſe to it on this occaſion with ſome little confidence. With the conſent of his officers, Gardner was carried to the public ſalt-water baths of this town, then of the temperature of 36° Fah^{t}. and thrown headlong into it. The good effects were inſtantaneous. As he roſe from the firſt plunge, and lay ſtruggling on the ſurface of the water, ſupported by two of his fellow-ſoldiers, we obſerved that he ſtretched out his left leg, which had been for ſome time retracted to the ham. But his head did not immediately recover the ſame freedom of motion, and therefore he was plunged down and raiſed to the ſurface ſucceſſively for upwards of a minute longer, the muſcles of the neck relaxing more and more after every plunge. When taken out, we felt ſome alarm: a general tremor was the only indication of life, the pulſe and the reſpiration being nearly, if not entirely, ſuſpended. Warm blankets had however been prepared, and a general friction was diligently employed. The reſpiration and pulſe became regular, the vital heat returned, the muſcles continued free of conſtriction, and the patient fell into a quiet and profound ſleep. In this he continued upwards of two hours, and when he awaked, to the aſtoniſhment of every one, he got up and walked acroſs the room, complaining of nothing but hunger and debility. The convulſive hiccup indeed returned, but in a ſlight degree, and gave way to the uſe of the cold bath; which he continued daily a fortnight longer; and in leſs than a month we had the ſatisfaction of ſeeing our patient under arms, able and willing for the ſervice of his country.

That the opium, though it failed in affecting a cure, had conſiderable influence in mitigating the diſeaſe, and prolonging life, is, I think, apparent.

That the mercury had little effect is clear from the ſecond convulſions coming on ſoon after the ſalivation appeared.

The ſucceſs of the cold bath in circumſtances ſo apparently deſperate, was not leſſened by bad effects of any kind. Though the patient was in a ſalivation when thrown into it, yet this was not ſtopped ſuddenly; it leſſened indeed immediately, and ſoon diſappeared, but without any of thoſe bad conſequences ſo well deſcribed by Sir John Sylveſter and Dr. Dobſon. Subſequent experience has taught me to attribute ſome part of the ſuddenneſs of the benefit obtained in this inſtance to a circumſtance that diſtreſſed me much at the moment. The very inſtant that we were about to immerge poor Gardner, he was ſeized with a general convulſion. We heſitated, but kept our purpoſe, and happily plunged him into the water with the convulſion upon him. I am alſo inclined to think that our ſucceſs is in part to be aſcribed to the powerful, general, and ſudden application of the remedy, and under this opinion I ſhould prefer immerſion in water, to its affuſion, the method employed by Dr. Wright.

2. Soon after this I was ſent for by a poor woman, who, in conſequence of difficult labour, and, as ſhe imagined, of local injury in ſome part of the uterus, was ſeized with the ſpaſmus cynicus, locked jaw, and other ſymptoms of tetanus. She was immediately taken to the cold bath, and thrown into it in the ſame manner as the former patient, and with ſimilar good effects. The ſpaſms diſappeared, and though they afterwards returned in a ſlight degree, they gave way entirely to a ſecond immerſion.

In the firſt of theſe caſes I was aſſiſted by Mr. Walker, ſurgeon's mate of the Staffordſhire militia; and the ſecond caſe the effects of the cold bath were witneſſed by him and my friend Mr. Park.

So far of the idiopathic tetanus; my experience of the effects of the cold bath in the diſeaſe originating in wounds is neither ſo ſatisfactory nor ſo complete.

3. The firſt caſe that occurred to me was that of a maſter of a veſſel returning from ſea in the year 1784. The injury had been received on the leg, but the wound

had little inflammation on it, though the patient was ſo far advanced in the diſeaſe as to go off in a general convulſion, before any remedy could be tried.

4. The ſecond caſe ſeemed rather an inſtance of locked jaw, properly ſo called, than of tetanus. The injury having been received on the jaw itſelf, the affection was confined to the muſcles of that part, and the diſeaſe ſeemed to be the triſmus inflammatoria of Sauvages. It gradually yielded to bleeding and the topical application of warm vapour, the cold bath producing no good effect.

5. In the third caſe we contented ourſelves with pouring cold water in ſucceſſive buckets on the patient's head, partly becauſe this was the moſt convenient method, and partly becauſe it is that employed by Dr. Wright, whoſe paper on the ſubject in the ſixth volume of the Medical Obſervations and Inquiries, had by this time appeared. While we were buſy in this way, and flattering ourſelves with ſome appearance of ſucceſs, the unhappy patient ſuddenly threw himſelf on his back, and a quantity of water entered his mouth. The effects were highly diſtreſſing. The muſcles of deglutition were unable to carry down or to expel the fluid, and ſuch convulſions took place as led us to expect inſtant death. He recovered, however, ſo far as to regain his former ſituation, but the effects of his ſufferings on the bye-ſtanders prevented our continuing this practice, and he was carried off a few hours after.

6. A fourth inſtance of this diſeaſe I ſaw about two years and a half ago. There was no room for new practice, for the patient was too far advanced. He had been treated with opium and mercury as uſual, and as uſual died.

Theſe caſes afford little inference either in favour of or againſt the uſe of the cold bath in tetanus ariſing from wounds, but they are ſtated ſhortly, that the account of my experience in this diſeaſe may be fair and complete.

It does not appear from Dr. Blane's Hiſtory of the Diſeaſes of the Fleet, that this remedy was uſed in any of the caſes of tetanus that occurred among the ſeamen in the late war. In ſpeaking of this diſeaſe Dr. Blane indeed mentions, that though Dr. Wright and Dr. Cochrane had found the cold bath ſucceſsful in caſes without local injury, they had acknowledged the practice to be ineffectual in the diſeaſe when ariſing from wounds. Dr. Blane may perhaps be accurate reſpecting Dr. Cochrane, whoſe publication on this ſubject I have not been able to procure, but he is miſtaken reſpecting Dr. Wright. This gentleman, in the paper already quoted, informs us, that ſince he employed the cold bath, he has not failed in effecting a cure in a ſingle inſtance of tetanus, without diſtinction or reſervation; and of the ſix caſes which he has detailed, in three the diſeaſe appears to have ariſen from local injury. This point is of high importance, and a miſtake reſpecting it in a work like that of Dr. Blane's, which will no doubt be much conſulted in the future practice of the fleet, may, if paſſed unnoticed, have ſerious conſequences.

It is worthy of remark, that the uſe of the cold bath in tetanus is a practice of very ancient date. Dr. Wright, whoſe preciſion and candour are exemplary, mentions that he received the firſt hint of it from Dr. Lind of Haſlar. Hippocrates, however, was acquainted with it, as appears by Aphor. 21. lib. 5. It is alſo noticed by Avicenna, lib. 3. cap. 7. and in the collection of Schenkius, p. 120, an account of the ſucceſs of this practice in two caſes of tetanus is quoted from *Jaſon*, cap. 21. *De morbis cerebri, ex Valeſco.* Hippocrates indeed, and after him Avicenna, reſtrict the uſe of this remedy to the young and corpulent, and to the ſummer ſeaſon. They alſo confine it to caſes in which the diſeaſe has not originated in local injury, for it appears, by another of his aphoriſms, that the father of phyſic thought convulſions ſupervening to a wound, always fatal*. The experience of Dr. Wright encourages us to extend this remedy to tenatus originating in wounds, but if it ſhould fail we are ſtill not to deſpair, as the following caſe will teſtify.

7. In July, 1787, a labouring man was brought into our Infirmary with a wound in his hand. He was under my friend Mr. Park's care, and the ſore was nearly healed, when an alarming rigidity appeared in the motion of the jaw. Opium and mercury were immediately preſcribed, with which were conjoined firſt the cold, and afterwards the warm bath. Mr. Park, however, finding the diſeaſe to proceed with the moſt unfavourable omens, called a conſultation, at which, in the abſence of the reſt of our colleagues, we agreed to make a trial of bark and wine on the authority of Dr. Ruſch of Philadelphia, whoſe excellent paper on this ſubject, in the ſecond volume of the American Philoſophical Tranſactions, had juſt appeared. We had, however, little or no hope, for the diſeaſe was far advanced, and its progreſs had been very rapid. At this time the jaw was not only rigidly contracted, but the ſpaſms had extended to the neck and back, the pain under the enſiform cartilege was moſt acute, and twice or thrice in every hour he was ſeized with general convulſions, each of which laſted about half a minute. The

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* Hippoc. Aph. lib. iv, ſect. v. aph. 2.

The patient was, however, a man of a vigorous mind, and in his perfect ſenſes; and his danger was not concealed from him. Death, he was told, muſt be the inevitable conſequence, unleſs he ſwallowed wine in large quantity, but this we verily believed would ſave him. At firſt bark was infuſed in the wine, but he could not ſwallow the mixture, and therefore we truſted to wine alone. It was wonderful to ſee the exertions which this poor fellow made. If the liquid was offered to him at an improper time, the effort of deglutition brought on a general convulſion; nay a general convulſion was the conſequence of advancing it at ſuch a time towards his head. But watching the remiſſion of the ſpaſms, he was able to ſwallow a table ſpoonful or more at once, he himſelf giving the ſignal when the wine ſhould be adminiſtered.* In this way, through the opening made by the lapping of the upper jaw over the under one, he drew up and ſwallowed a quart of port wine in the courſe of two hours; at the end of which time he thought himſelf refreſhed, and was encouraged to proceed. At the end of twenty-four hours he had finiſhed his third bottle, and at this time it was evident that the down-hill progreſs of the diſeaſe was checked. Though the pain under the enſiform cartilege (the moſt excruciating of all the ſymptoms of tetanus) was little abated, yet he felt himſelf more able to bear it; and the general convulſions were certainly leſs frequent and leſs ſevere. We continued our plan with patience and vigour, but for a long time life and death ſeemed to hang in equal ſcales. On the fifteenth day of this courſe he was affected with a ſlight nauſea and vomitting, vomiting, which ſoon went off, and thenceforward his recovery ſeemed more apparent. It was not however till he had been forty-two days under this treatment that his ſafety could be aſcertained, and during this time he ſwallowed a hundred and ten bottles of Port wine.

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* This circumſtance ſuggeſted to me the ſimilarity between this caſe and hydrophobia. I noticed it to my reſpected friend Dr. Percival, who has mentioned it in his valuable eſſay on that diſeaſe. V. Percival's Eſſays, vol. II. p. 366. 4th edition. I differ however from this experienced phyſician in his ſuppoſition that the two diſeaſes are the ſame, or of the ſame origin.

In this caſe bark was adminiſtered along with the wine towards the latter period of the complaint ; and during the whole of it, gruel or milk in the quantity of a quart a day, or upwards, was given by way of nouriſhment. The milk, however, he preferred, as agreeing better with the wine, and tending, as he conceived, to allay a burning ſenſation at his ſtomach, the effect of an acid ferment there.

Though the wine was given in ſuch quantity, yet it never produced any ſymptom of ebriety—it ſoothed the irritation of his nerves, and comforted his mind, and, without increaſing the frequency of his pulſe, it augmented his ſtrength.

Every night he took from ſixty to a hundred and twenty drops of the tincture of opium, and, with the wine, this ſmall doſe ſeemed to have a more compoſing effect than thrice the quantity taken before he began it.

To allay the pain under the enſiform cartilege, clothes dipped in æther were applied externally with good effects. As I ſtood by him one day, preſſing the doſſils with my open hand to the ſcrobiculus cordis, he expreſſed himſelf relieved by the force which I applied. The ſame relief ſeemed to follow from a gentle and uniform preſſure on the parts ſuffering from ſpaſm. In conſequence of this, rollers were applied round each thigh where ſpaſmodic twitchings had been very diſtreſſing, and afterwards, at his own requeſt, a general preſſure of the ſame kind was applied to the back and abdomen. Theſe bandages he would not ſuffer to be removed, but we poured æther on them occaſionally, over the parts moſt affected by ſpaſm, guarding in the uſual way againſt the cold produced by too ſpeedy an evaporation.

At length our patient recovered. He is now a watchman, and calls on me occaſionally. He complains of a ſtiffneſs at times in the muſcles of his back, and though naturally a ſtrong and healthy looking man, his features retain the indelible impreſſions of his diſeaſe. His eyes appear hollow, his face ſharp and pale, his cheeks and lips are ſkinny, and the maſſiter muſcles hard and ſhrivelled.

I have lately had an account of a caſe of this kind which occurred in a ſailor on the coaſt of Guinea, who recovered under the care of Mr. Robſon, an ingenious ſurgeon in the African trade, from a ſtate apparently deſperate, by a very liberal uſe of bark and ardent ſpirits.

To conclude this part of my ſubject, I have known two inſtances of this diſeaſe in horſes, both originating in wounds; the firſt of theſe was treated in the method firſt deſcribed. The cold bath was uſed repeatedly, and large doſes of opium employed in the form of clyſters; but the diſeaſe terminated fatally. The ſecond of theſe occurred in a horſe of my friend Dr. Camplin (whoſe aſſiſtance in the caſe of the watchman I ought to have acknowledged), about a twelve-month ago in his journey to Bath. The doctor informs me that he gave him wine and opium, mixing ſixty drops of laudanum with every pint of wine, and by this practice he had the pleaſure of recovering him, though not till he had drank as much wine as he was worth. Brandy or gin may perhaps be as ſucceſsful in ſuch caſes.

It may be ſuppoſed that theſe inſtances of ſucceſs in the uſe of the cold bath and wine in tetanus, would lead me to the employment of theſe remedies in other convulſive diſorders. This has accordingly been the caſe; but of the uſe of wine I am not yet prepared to ſpeak. Of the uſe of the cold bath in ſuch diſorders I can ſpeak with ſome confidence, as my experience of it is now of eight years' duration.

In the convulſions of children I have found the cold bath a moſt uſeful remedy, whether the diſorder originated in worms or other cauſes. I have ſeldom known it to fail in ſtopping the paroxyſms, at leaſt for ſome time, and thereby giving an opportunity of employing the means fitted to remove the particular irritation. I muſt however obſerve, that in early infancy I have uſed it with caution; ſometimes tempering the water when the weather was cold, and ſometimes pouring it on the patient, rather than immerſing the patient in it. I have in general made the application of cold in this way ſudden and tranſient, have employed means to ſecure re-action, and have avoided the remedy entirely in all caſes where the vital energy ſeemed much exhauſted. With theſe precautions I have ſeen great ſucceſs attend this remedy in a variety of inſtances. Minutes of nine of theſe I have preſerved, but I mean to give one only in detail.

John Slater, aged eight years, came under my care in the month of January, 1782. About two years before, while at play, he was ſeized ſuddenly with a convulſion which continued for half an hour, and had returned ever ſince at ſhort intervals. Various means had been employed for his relief but without ſucceſs; the fits were become more and more frequent a hemiplegia had ſupervened, and the intellect was apparently loſt. For twenty-four hours he had lain in a ſtate of inſenſibility, motionleſs on the left ſide, and the muſcles of the other ſide only agitated by the convulſive tremors. We put him immediately into a tub of cold water, which inſtantly ſtopped the paroxyſm and threw him into a deep ſleep, out of which he awaked after two hours with a ſhriek, and fell into convulſions as before. The cold bath was repeated, and afterwards continued daily, preſent relief being always obtained by it. The interval was employed in adminiſtering the tin powder, which was worked off with calomel, but no worms were obſerved to be diſcharged. After ſeveral days the convulſions returning, though with a conſiderable abatement, I became diſſatisfied with the mode in which the bathing had been performed, the ſize of the tub employed never having admitted of ſudden immerſion. On this account we had the child conveyed to the public bath, into which he was thrown headlong, his father being ſtripped in the water to receive him. The temperature of the bath was 43° Farenheit. He was repeatedly plunged down and taken up for half a minute; was taken out of the water free of convulſion, fell immediately into a profound ſleep, and awaked clear of complaint. In theſe reſpects this caſe reſembles that of Gardner, firſt deſcribed. It reſembles it alſo in this reſpect, that convulſion had taken place at the moment of immerſion. The paralytic weakneſs of the ſide remained, however, for ſome time, but by the continued uſe of the bath it was at length entirely removed; and the powers of the mind, which had been totally ſuſpended, gradually returning, at the end of ſix months were perfectly reſtored.

He continued well upwards of twenty months, was healthy and vigorous in body, and in the acquirement of knowledge remarkably acute. But on the 30th of December, 1783, his mother having ſtruck him and frightened him much, he was ſeized in the night during ſleep with general convulſion, in which he continued ſeveral hours without intermiſſion. Being again called to him, I employed the ſame remedy, but at the time of immerſion the convulſion was not on him. Sleep and refreſhment followed as before, but the paroxyſm returned in the evening, though in a ſlighter degree. However, by perſeverence in the daily uſe of the bath, and throwing him twice into the water with the *convulſions upon him*, in ſix days his health was reſtored.

I lament over the fate of this amiable boy. He continued from this time three years and a half in perfect health; grew ſtrong and tall, and ſhewed great affection of temper and ſenſibility of mind. But being violently agitated with fear and grief, he fell again into convulſions in the month of July, 1787. I was ſent for as uſual, but was abſent from town at a conſiderable diſtance. Having on former occaſions launched him myſelf into the bath, his mother, a ſuperſtitious woman in a low walk of life, would not permit this remedy to be uſed till my return. He lay for thirty hours convulſed and ſenſeleſs, and the firſt intelligence I had of his illneſs was accompanied by the account of his death.

This caſe differs materially from the convulſions of early infancy. I give it therefore not as an inſtance of theſe, but as a ſpecimen of a diſeaſe affecting children from four to twelve years of age, and which, though not very uncommon, as far as I can judge, has not yet found its place in any ſyſtematic work that I know of. The noſology of convulſive diſeaſes is indeed very imperfect, and the terms we employ in deſcribing them are by no means preciſe. Thoſe who have ſeen tetanus and the diſeaſe laſt mentioned, will know, that what is called convulſion in both inſtances, is in the one caſe and the other a very different affection. The convulſion of tetanus is a ſudden and violent contraction of all the voluntary muſcles, throwing the frame into ſtrong contortions, but laſting only a minute at moſt, and ſeldom ſo long. The patient, where the caſe terminates fatally, dies in it at laſt, from the ſpaſmodic affection extending to the heart, or ſometimes perhaps, from the ſuſpenſion of reſpiration. The convulſion in the other caſe, though it begins with violence, ſoon ſubſides, leaving the body in a ſtate approaching to coma; and though the ſeparate fibres of many of the muſcles keep in conſtant vibration, and the turbulence of general convulſion returns occaſionally, yet in the intervals the reſpiration is natural, and the pulſe little diſturbed; voluntary motion and conſciouſneſs, are, however, completely ſuſpended.* If this ſtate continues long, the violent commotions return more frequently, the breathing becomes laborious, the ſtrength is at length exhauſted, and the patient dies with the ſymptoms of apoplexy.

I conclude this paper, already too long, with a few general remarks.

1. It appears to me that the efficacy of the cold bath in conclusive diſorders is much promoted by being em-

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* This ſpecies of convulſion has been denominated *clonic*, in oppoſition to *tonic*, but with little propriety. All convulſions may be ſaid to be clonic.

ployed during the preſence of convulſion. How I came to be led into this opinion may be ſeen in the caſe of Gardner. Much experience is required to eſtabliſh this as a general truth, and I give it as an opinion only. Yet it is an opinion ſo far confirmed in my mind, that, where left to my own judgment, I ſhould act upon it in all ſuch caſes as I have deſcribed; and, ſhould occaſion require, I ſhould be anxious to have the experiment tried in my own perſon, and "to run the hazard of the die." As madneſs is, I think, beſt combated in the height of the phrenzy, ſo I believe convulſive diſeaſes are moſt capable of remedy in the agitation of convulſion. I ſeem to have a glimpſe of a general principle in nature that connects theſe facts ſo apparently different; but the ground is not yet firm enough to lay the foundation of a theory.

2. It may however ſeem to ſtrengthen the above remark, that in ſpaſmodic diſeaſes, which riſe not to general convulſion, the cold bath ſeems to be of inferior efficacy. In chorea Sancti Viti I have tried it frequently, but never found it of any ſervice. This is one of the few diſeaſes in which electricity is of decided advantage.

3. In the hyſteric paroxyſm the cold bath, or indeed the plentiful affuſion of cold water, is an infallible remedy. Thoſe who ſuppoſe that the terror it occaſions ought, in this caſe, to prevent our having recourſe to it, are, in my opinion, miſtaken. Though the hyſteric paroxyſm be the offspring of paſſion, it is never occaſioned, I will venture to aſſert, by the paſſion of fear. A ſenſe of danger will always, I believe, prevent it, or indeed a powerful dread of any kind. I have known a tub of cold water kept in readineſs, with the certainty of being plunged into it on the

recurrence of the paroxyſm, cure this diſeaſe, without the remedy being ever actually tried. I know the hyſteric paroxyſm often takes place when *danger is over*, but that is another caſe.

4. I have tried the cold bath in the epileptic paroxyſm, but my experience of its effects is as yet too recent and too imperfect to be detailed. I am not ſo ſanguine as to expect that experience will accumulate rapidly on this ſubject. Many there are who will think my practice too hazardous to be copied, even after the account of its ſucceſs. This relates eſpecially to the uſe of the cold bath in convulſive diſorders in private practice; but in hoſpital practice, and in the practice of the fleet and army, the ſame objections do not preſent themſelves; and in theſe departments of the profeſſion it may be expected that its effects will be aſcertained.

Dr. Girdleſtone, in his account of the diſeaſes among the troops in India, mentions that hot Madeira is given with ſucceſs in the tetanus which occurs in that climate; but he tells us, though not from his own experience, that the cold bath has been very unſucceſsful. To what circumſtance it may be owing that a remedy, which has been ſo efficacious in the Weſt Indies, ſhould have failed in the Eaſt, does not appear; though if the circumſtances under which it was employed were fully related, the difficulty might perhaps be explained. It may, however, be obſerved, that in the ſultry climate of India, where the human frame is greatly relaxed, it will not be ſafe to uſe the cold bath with the ſame freedom as in more northern regions. And it will undoubtedly be more ſafe, in any future trials of this remedy in that country, to uſe the method of affuſion employed by Dr. Wright and ſo ſucceſsful in his practice in a ſimilar climate, inſtead of immerſion, which it appears was employed in the caſes mentioned by Dr. Girdleſtone.

I preſent theſe facts and obſervations to you, Gentlemen, with the confidence of a man whoſe intentions are good. I have not ſpoken under the warmth of new conceptions, for my experience is, you ſee, of ſeveral years' duration; and I anxiouſly hope that time will ſtamp my narration with the ſeal of truth. Be this as it may, I have written under the ſtrong impreſſion of the folly, baſeneſs, and guilt, of ſacrificing the intereſts of ſcience to a ſelfiſh purpoſe; and if I am in any reſpect inſtrumental in deceiving others, I ſolemnly declare it is becauſe I myſelf am deceived.

N°. II.

An Account of the remarkable Effects of a Shipwreck on the Mariners; with Experiments and Obſervations on the Influence of Immerſion in freſh and ſalt Water, hot and cold, on the Powers of the living Body.

Read before the Royal Society, April 19, 1792, and extracted from Part I. of their Tranſactions for that year.—Page 199.

THE following narrative is ſubmitted to the Royal Society, as containing in itſelf ſome curious circumſtances, and as having ſuggeſted the experiments afterwards to be recited.

On the 13th of December, 1790, an American ſhip was caſt away on a ſand-bank that lies in the opening of the river Merſey into the Iriſh Channel. The crew got on a part of the wreck, where they paſſed the night; and a ſignal which they made being diſcovered next day from Hillberry Iſland, a boat went off, though at a great riſk, and took up the ſurvivors. The unfortunate men had remained twenty-three hours on the wreck; and of fourteen, the original number, eleven were ſtill alive, all of whom in the end recovered. Of the three that periſhed one was the maſter of the veſſel; another was a paſſenger who had been a maſter, but had loſt or ſold his ſhip in America; the third was the cook. The bodies of theſe unfortunate perſons were alſo brought off by the men from Hillberry Iſland, and were afterwards interred in Saint Nicholas church-yard, amidſt a great crowd of ſpectators. The cook, who was a weakly man, died a few hours before the boat reached the wreck, but the two maſters had been long dead, and this united the ſympathy for their loſs with a curioſity to inquire into its circumſtances and cauſes. When the following particulars came to be known, this curioſity was increaſed. Both the maſters were ſtrong and healthy men, and one of them a native of Scotland, in the flower of life, early inured to cold and hardſhips, and very vigorous both in body and mind. On the other hand, ſeveral of the ſurvivors were by no means ſtrong men, moſt of them had been inured to the warm climate of Carolina, and what was gular enough, the perſon among the whole who ſeemed to have ſuffered leaſt was a negro.

What is extraordinary is ſeldom long unaccounted for in one way or other, and the death of the two maſters was ſaid to have been owing to their having taken poſſeſſion of a keg which had contained cherry-brandy, and which ſtill contained the cherries;—theſe, it was reported, they had kept to themſelves, and eaten in large quantities after the ſhipwreck; and this having produced intoxication, was ſuppoſed to have haſtened their death. Some experienced ſeamen were ſatisfied with this account, which indeed ſeemed very rational; for though ſpirituous liquors may fortify the body againſt the effects of heat combined with moiſture, and may perhaps ſupport it for a ſhort time under great fatigue, they are, I believe, uniformly hurtful when taken under ſevere and continued cold. Pleaſed to ſee a doctrine becoming popular which has been ſo ably ſupported by Dr. Aiken,* and others, I believed it might receive a ſtriking confirmation from this cataſtrophe, into the particulars of which I determined to examine accurately. I therefore obtained acceſs to the ſurvivors of the crew, and from them, but more eſpecially from Mr. Amyat, the mate, I received the information which I required.

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* See Tranſactions of the Philoſophical and Literary Society of Mancheſter, v. I.

In repeated converſations with this intelligent young man, I learnt that Capt. Scott, the maſter of the veſſel, died in about four hours after the ſhip ſtruck; and that Capt. Daviſon, the paſſenger, died in about ſeven: but that the ſtory of their having eaten cherries infuſed in brandy was entirely without foundation: of this he was certain, for he ſaw the keg, which contained the cherries, ſtaved, while Capt. Daviſon was endeavouring to fill it with water to make grog for the crew; the cherries fell on the wreck, and were immediately waſhed into the ſea. Mr. Amyat expreſſed his ſurpriſe at the early death of the two maſters, but could not aſſign any cauſe for it. He ſaid there was no liquor of any kind ſaved, nor any ſort of food; that the whole crew were on an equality in all points, except that ſome were deeper in the water than others, but that the two maſters had the advantage in this reſpect, for they ſat on the only part of the wreck that was out of the ſea, whereas the poor negro, who eſcaped almoſt unhurt, was perhaps deepeſt in the ſea of any. He explained this in the following manner. When the ſhip ſtruck they cut away her maſts to prevent her from overſetting, and after this ſhe drifted over the ſand bank, into what he called a "ſwaſh" on the other ſide. Here ſhe floated, and they let go their beſt bower anchor; but it dragged, and the veſſel ſtruck again in a few minutes on another bank. In this ſituation ſhe lay ſome time, beating againſt the ſand, and the ſea breaking over her. In a little while Mr. Amyat ſaw the tar barrels, which formed her cargo, floating towards the land, and ſoon after the bottom parted entirely, and was carried in the ſame direction. Happily for the men, the part of the wreck on which they were laſhed was held by the anchor, and floated in the water, a ſmall portion of the after part of the quarter-deck being above the ſurface. On this ſat the two maſters, generally out of the ſea, but frequently overwhelmed by the ſurge, and at other times expoſed to heavy ſhowers of ſleet and ſnow, and to a high and piercing wind. The temperature of the air, as nearly as can be gueſſed, was from 30° to 33° of Fah^{t}. and that of the ſea, from trials in ſimilar circumſtances, from 38° to 40°. Immediately before the two maſters was Mr. Amyat himſelf. As he was ſitting, and the deck ſloped pretty rapidly, he was generally up to the middle in the water. The ſituation of the reſt may be ſuppoſed; ſome of them were up to the ſhoulders. They were not at any time able to change their poſition, but kept their legs in pretty conſtant motion to counteract the cold, their arms being employed in holding by the wreck.

The maſter of the ſhip, Capt. Scott, a native of North-Carolina, and about forty years of age, died firſt. As they were in the dark, Mr. Amyat could not ſee his countenance; but he was firſt alarmed by hearing him talk incoherently, like one in the delirium of fever. By degrees his voice dwindled into a mutter, and his hearing ſeemed to fail. At length he raiſed himſelf up in a ſort of convulſive motion, in which he continued a few ſeconds, and then fell back dead on the deck. This happened about eight in the evening; four hours after the ſhip went aground.

Soon after this, Capt. Daviſon, who was about twenty-eight, began to talk incoherently, in the ſame manner as the other. He ſtruggled longer, but died in the ſame way, at about eleven at night. The cook died in the forenoon of the ſucceeding day. He was a low-ſpirited man, and deſponded from the beginning. All the reſt held out, as has been already mentioned, though ſorely pinched with cold and hunger, till they were taken up about three in the afternoon. Mr. Amyat ſaid that his hands and feet were ſwelled and numb, though not abſolutely ſenſeleſs; he felt a tightneſs at the pit of his ſtomach, and his mouth and lips were parched; but what diſtreſſed him moſt was cramps in the muſcles of his ſides and hips, which were drawn into knots. Though immerſed in the ſea, they were all of them very thirſty; and though expoſed to ſuch ſevere cold, Mr. Amyat himſelf was not drowſy, nor were any of the men drowſy, nor did ſleep precede death in thoſe that periſhed. Theſe facts are curious.

Reflecting on the particulars of this melancholy ſtory, there ſeemed no doubt that the death of the two maſters was to be imputed to their peculiar poſition on the wreck. Expoſed to heavy ſhowers of ſleet and ſnow, they might ſuffer from being wet with freſh, rather than ſalt water: they might alſo ſuffer from being expoſed to the cold of the atmoſphere, probably ſeven or eight degrees greater than that of the ſea. The chilling effects of evaporation might operate againſt them, promoted as theſe muſt have been by the high wind; or they might receive injury from their frequent immerſions in the ſea, producing an *alternation* in the media ſurrounding. This laſt ſuppoſition did not, indeed, ſtrike me at this time; the others dwelt on my mind.

Of the powers attending animation, that which ſeems fundamental, is the capacity of the living body of preſerving the ſame heat in various degrees of temperature of the ſame medium, and, indeed, in media of very different denſity and preſſure. If a definition of life were required, it is on this faculty that it might beſt be founded. It is known that ſome fluids, applied to the ſkin, vary in their effects according to their impregnation. In the ſame degree of temperature, pure water on the ſurface of the body is much more hurtful than water in which ſalt is diſſolved. Seafaring men are univerſally acquainted with this, and a ſtriking proof of the truth, as well as of the importance of the obſervation, may be found in the Narrative of Lieut. Bligh. Probably the ſaline impregnation may ſtimulate the veſſels of the ſkin in ſome way that counteracts the ſedative or debilitating action of the cold. At any rate, it ſeemed not unlikely that ſome light might be thrown on this curious ſubject, by obſerving the effects of immerſion in freſh and ſalt water, of equal temperature, on the animal heat. And this might alſo aſſiſt in accounting for the death of the unfortunate men already mentioned.

EXPERIMENT I.

I placed a large veſſel, containing one hundred and ſeventy gallons of ſalt water, in the open air. The atmoſphere was damp, and what is called raw. The thermometer ſtood at 44° in the air, and this alſo was the temperature of the water. The ſubject of my experiment was Richard Edwards, a healthy man, twenty-eight years of age, with black hair, and a ruddy complexion. The hour choſen for his immerſion was four in the afternoon, about two hours after his dinner; a time appointed rather for my own convenience, than as being moſt proper for the purpoſe. His heat was 98° before undreſſing, his pulſe 100 in the minute. He was undreſſed in a room where the mercury was at 56°; and afterwards ſtood naked before the fire till his heat and pulſe were examined again, and found as before. He then walked pretty briſkly through a flagged paſſage into an open court, where the north-eaſt wind blew ſharply upon him: he was expoſed to it for a minute, and then plunged ſuddenly into the water up to the ſhoulders. The thermometer which had been kept in a jug of warm water, at the heat of 100°, was introduced into his mouth, with the bulb under his tongue, as ſoon as the convulſive ſobbings occaſioned by the ſhock were over. The mercury fell rapidly, and a minute and a half after immerſion it ſtood at 87°. He remained motionleſs in the water, and the mercury roſe gradually; at the end of twelve minutes it ſtood at 93°½. While he ſat in the water it occurred to me to examine his heat when he roſe out of it into the air: I had reflected on the power that muſt be employed to keep up his heat in a medium ſo denſe as water, and where an inanimate body, of the ſame bulk, would have cooled ſo much more ſpeedily than in air of the ſame temperature. Suppoſing that this heat-producing proceſs, whatever it may be, might continue its operations ſome time after the extraordinary ſtimulus (the preſſure of the water) was removed, I expected to ſee the mercury riſe by the accumulation of his heat, on changing the medium of water for air, and therefore kept him expoſed, naked, to the wind two minutes after taking him out of the bath. To my ſurpriſe, although the attendants were rubbing him dry with towels during this time, the mercury fell rapidly. He was put into a warm bed, and his heat when examined under the tongue, was 87°, at the axilla 89°. Frictions were uſed, and brandy mixed with water adminiſtered; but I found on this, as on all future occaſions, the beſt mode of counteracting the cold, was to apply a bladder, with hot water, to the pit of the ſtomach (the ſcrobiculus cordis), a fact which I think important: this being done, his ſhiverings, which before were ſevere, ſoon ceaſed, and he became more comfortable. Three hours afterwards, however, he had not entirely recovered his former heat; but by eight at night, he was in all reſpects as uſual.

I have been very minute in detailing the circumſtances under which this experiment was made; ſome of the particulars which, at the time, I thought of little conſequence, I found afterwards of importance. The experiment itſelf I determined to repeat as exactly as poſſible.

EXPERIMENT II.

On the next day, at the ſame hour, the ſame perſon was again immerſed, as before. His pulſe previouſly was 85, his heat 100°. He had been put to bed an hour before to ſave the time ſpent in undreſſing. The heat of the water and of the atmoſphere 44°. The wind north-eaſt, and ſtrong. On this occaſion, as before, there was a rapid fall of the mercury; the following table will ſave words:

	Ther.		Ther.
2 min. after immerſion	89° ½	9 min. after immerſion	95° ¾
3 —— - -	90 ½	10 —— - -	94 ½
4 —— - - -	92 ½	11 —— - - -	95
5 —— - -	94 ½	12 —— - - -	95
6 —— - - -	95	13 —— - - -	95 ½
7 —— - -	95 ¾	14 and 15 —— -	95
8 —— - - -	95 ¾		

At the end of fifteen minutes he was taken out, and ſtood three minutes, naked, expoſed to the north-eaſt wind, at the end of which time the mercury had ſunk to 88°. A draught of ale was given him, and he was put into a warm bed; in three minutes afterwards the mercury roſe to 93°. An hour after his heat was 95°.

The effects produced by this alternate expoſure to water and air of the ſame temperature, gave a new direction to my thoughts, and determined me to inquire again into this ſingular phenomenon. The moſt obvious method would have been to have prolonged the proceſs of alternation, and replunged the perſon cooled by the external air into the bath; but this was running too great a riſque, unleſs ſome more ſudden and certain method could be found of reſtoring the heat that might be loſt. It was prudent, therefore, to proceed more cautiouſly. In the next experiment I reſolved to try the methods of heating as well as cooling the body.

EXPERIMENT III.

On the following day, at the ſame hour, the ſame perſon was again immerſed in the ſalt water bath. His heat previouſly was 98°, his pulſe 100. The temperature of the air and the atmoſphere, as before 44°. The mercury ſunk rapidly to 90°.

2 minutes after	88°	10 minutes after	94°½
3 ——	88	11 ——	94 ¾
4 ——	88 ½	12 ——	95
5 ——	90 ½	13 ——	96
6 ——	92	14 ——	96
7 ——	92	15 ——	96
8 ——	94	16 ——	96
9 ——	94		

He was now taken out, and ſtood in the wind three minutes, ſhivering violently. This circumſtance rendered it difficult to aſcertain exactly the fall of the mercury, which was, however, conſiderable. When examined in the room in which he undreſſed, it ſtood at 90°. He was now plunged into a freſh-water warm bath, heated to 97°½. What is very ſurpriſing, the mercury fell two degrees. The following table will ſhew the progreſs of the return of his heat.

1 min. after immerſion in the warm bath, mercury,	88°	5 minutes after	94°
2 minutes	92	6 ———	96
3 ———	92	7 ———	96
4 ———	94	8 ———	96
		9, 10, 11, 12, to 16,	96

If the riſe of heat in the cold bath at 44°, and the warm bath at 97°½, be compared, the firſt will be found more ſlow; but that after being ſixteen minutes in the one and in the other, the heat was the ſame in both caſes, when taken at the mouth. It muſt, however, be acknowledged, that in the cold bath, the extremities were chilled and cold, while in the hot bath, the heat was equally diffuſed. When Edwards got out of the hot bath, he put on his clothes, and was remarkably alert and cheerful the whole evening. Encouraged by the ſafety of theſe experiments, I reſolved to increaſe the time of immerſion in the cold bath, and to inquire more generally into its effects on the ſenſations, as well as heat.

EXPERIMENT IV.

At the ſame hour of another day, the ſame perſon was again immerſed as before, his heat being previouſly 97° ½, and that of the water 42°. Wind north-eaſt, and briſk.

1 minute after,	heat 90°	12 minutes	
2 minutes	92	13 ———	
3 ———	92	14 ———	94° ½
4 ———	92 ¼	15 to 24,	94 ½
5 ———	92	25 ———	94
6 ———	92 ½	26, 27	
7 ———	94	28 ———	94 ½
8 9, 10, 11	94	29, 30	94

It will be obſerved, that in the above table there are blanks left in the report. At ſuch times the thermometer was taken out of Edwards's mouth, to admit of his anſwering the queſtions put to him. He ſaid, that on plunging into the water he felt an extreme cold, which he could not but think was partly owing to his being expoſed, naked, to the wind before; that this cold diminiſhed, and in a little while he felt comfortable, but that after a while the ſenſe of coldneſs returned, though leſs than at firſt; diminiſhing again, but in a leſs degree. At length his ſenſations became pretty fixed. In this ſtate, when the water was at reſt, he ſhould not even have known, by his feelings from the upper part of his cheſt to the pubes, that he was in water at all. His feet and legs were very cold; ſo were his hands and arms; and ſo alſo the penis and ſcrotum. He mentioned, likewiſe, that he felt a cold circle round the upper part of his body, though not conſtantly. On examining into this, I found it was greateſt at firſt, and that it extended over the ſpace which, from the undulations left in the bath by the plunge of immerſion, was alternately above and under the ſurface of the water: when the bath ſettled, it was little felt; but by agitating the fluid, I could reproduce

produce it at any time when the cold in the extremities was not ſo great as to prevent its being felt. This curious particular ſerves to explain a circumſtance much dwelt on by Mr. Amyat, in giving an account of his ſufferings on the wreck; that what he felt moſt ſeverely was the cramps in the muſcles of his hips and ſides, parts which, from his ſituation on the wreck, already deſcribed, muſt have been alternately under and above the ſurge. Here I muſt obſerve, that the ſea did not break over the ſufferers all the time they were on the wreck. The wind moderated, as well as the waves, and for the laſt fifteen hours, they were not at any time overwhelmed, or at leaſt Mr. Amyat himſelf was not. The cold never abated. Being all laſhed to the wreck, they never changed their poſitions; the bodies of thoſe who died occupied the ſpace where they were originally placed. Mr. Amyat, therefore, during the whole time ſat nearly up to the middle in water, but ſubject to the variations occaſioned by the motion of the ſea.

To return.—When expoſed naked to the wind, the mercury, in this caſe, ſunk as uſual five or ſix degrees, and his ſhiverings were great. Deſirous of reſtoring his heat as ſpeedily as poſſible, we incautiouſly heated the hot bath to 104°: but after being half a minute in it, he ſcreamed out with pain, eſpecially in his extremities, and about his ſcrotum. When taken out, his ſhiverings almoſt amounted to convulſion. The bath was lowered to 88°, and he was replaced in it, and its temperature progreſſively, but pretty rapidly increaſed to 100°. He continued, however, to ſhiver much, his heat remaining about 90°; but a bladder, with very hot water, being introduced under the ſurface of the bath, and applied cloſe to his ſtomach, the good effects were inſtantaneous, his ſhiverings ceaſed, and his heat mounted rapidly to 98°.

All theſe experiments having been made on one perſon, I determined to repeat this laſt on another.

EXPERIMENT V.

R. Sutton, aged 19, of a pale complexion, and a feebler frame, was immerſed in the bath, under the circumſtances of the preceding experiment. His heat was previouſly 96°½.

½ a minute after, heat	92°	18 minutes	93°¼
1 minute	90	19 ———	93½
2 minutes	88½	20, 21	94
3 ———	89	22 ———	92½
4 ———	90	23 ———	92¼
5 ———	92	24 ———	92¼
6 ———	92¼	25 ———	94
7 to 10	92	26 ———	94
11 ———		27 ———	92½
12 to 15	92	28 ———	92¾
16 ———	92½	29 ———	94
17 ———	93	30 ———	94

Though this perſon ſeemed to bear the cold bath well, having loſt in thirty minutes only 2½ degrees of heat, yet when expoſed afterwards to the wind, he ſhivered violently, and loſt his heat very faſt. He was put into a warm bath, heated to 96°, but recovered his heat very ſlowly, as the following table will ſhow.

1 minute after, heat	88°
2 minutes	90
3 ———	90½

4 ———

4 ———	90°	great ſhivering.
5 ———	90	here the bath was heated to 100°.
6 ———	90	ſhiverings ſtill.
7 ———	90	ditto.
8, 9	90 ½	ditto.
10 ———	92	ditto.
11 ———	92	bath heated to 104°.
12 ———	94	
13 ———	93	——— heated to 108°. Shiverings.
14 ———	93	a bladder with very hot water applied to the ſtomach.
15 ———	94	
16 ———	96	very comfortable.

EXPERIMENT VI.

Richard Edwards, the original ſubject of experiment, was again immerſed in the cold bath, of the temperature of 40°, and remained in it three quarters of an hour. His heat previouſly was 97° his pulſe 90 in the minute. The mercury fell at firſt to 92°, was ſtationary for a few minutes, and then mounted, though as uſual, with no regularity. In twenty-two minutes it ſtood at 96°; it then began to decline, and in twenty-three minutes more had ſunk to 94°. Being expoſed as uſual to the wind, the mercury ſunk as uſual, and he ſhivered violently. In the warm bath at 96° his ſhiverings continued ſeveral minutes, his heat remaining at 90 and 91°. In ſeven minutes the mercury began to riſe faſt, and five minutes after was at 96°.

EXPERIMENT VII.

The effects of forty-five minutes immerſion in the cold ſalt-water bath, at 40°, were propoſed to be tried on Richard Sutton. He was much under the impreſſions of fear, and his heat previouſly raiſed the mercury only to 94°. The mercury ſunk, as uſual, on his immerſion, but to an unuſual degree. It did not ſtop in its fall till it got to 83°, which perhaps might in part be accounted for by the extraordinary chattering of his teeth, admitting ſome contact of the air. It then mounted in the uſual irregular way, and at the end of thirteen minutes had got to 92°. Here it ſtood for nineteen minutes longer with little variation; at the end of this time it began to fall rapidly, though irregularly, and in three minutes was down at 85°. He had now been thirty-five minutes in the water, and I did not think it ſafe to detain him longer; we therefore hurried him into a warm bath, heated to 96°, where he ſhivered much. The bath was heated gradually to 109°, and in this heat he recovered his proper temperature in about twenty-eight minutes. Being then put into a warm bed, he fell into a profuſe perſpiration, which left him in his uſual health.

One general remark will ſerve for the pulſe in all theſe experiments. It was not poſſible to keep the ſubjects of them from ſome degree of previous agitation, and this always quickened the pulſe. The natural pulſe of Edwards was about 70 in the minute; but it may be obſerved, that it was never ſlower than 85 before immerſion, and generally more. However this might be, it invariably ſunk to 65, or from that to 68, in the water; became firm, regular, and ſmall. After being long in the bath, it could hardly be felt at the wriſt, but the heart pulſated with great ſteadineſs and due force. In the laſt experiment, when the heat ſunk rapidly, Sutton ſaid that he felt a coldneſs and faintneſs at his ſtomach, which he had not perceived before, and when I felt the motion of his heart, it was feeble and languid. In ſome future trials of the effects of immerſion in freſh water (one of which I ſhall detail) the ſame coldneſs at the ſtomach preceded a rapid fall of the mercury; and theſe facts, together with the effects I found from applying a conſiderable heat to this part when the body was chilled with cold, convince me that there is ſome peculiar connection of the ſtomach, or of the diaphragm, or both, with the proceſs of animal heat. Whoever will conſider the rapidity with which a dead body would have cooled immerſed in water of the temperature of 40°, may form ſome eſtimate of the force with which the proceſs of animal heat muſt have acted in the experiments already recited. Theſe experiments, however, do not coincide with our generally received theories of animal heat. The increaſe of heat, in fever, has led ſome perſons to believe that animal heat is produced by, or immediately connected with, the action of the heart and arteries; here, however, it may be obſerved, that while heat muſt have been generated in the bath with more than four-fold its uſual rapidity, the vibrations of the arterial ſyſtem were unuſually ſlow. Another, and a very beautiful theory of animal heat, ſuppoſes it immediately to depend on reſpiration; but in the bath, after the firſt irregular action of the diaphragm from the ſhock of immerſion was over, the breathing became regular, and unuſually ſlow. Laſtly, the curious phænomenon of the heat riſing, and falling, and riſing again, in the bath, with the body at reſt, and the temperature of the ſurrounding medium unchanged, is, I think, fatal to thoſe theories of animation which conſider the living body as a mere machine, acted on by external powers, but not itſelf originating action, and differing from other machines only in the peculiarity of the powers which are fitted to ſet it in motion.* I have ſaid that the temperature of the medium continued unchanged, but it may be ſuppoſed that the bath was heated a little during the experiments; it was ſo; but being expoſed, with a large ſurface, to the open air, the wind blowing briſkly over it, its heat was little altered; in twelve minutes immerſion it had gained nearly one degree, and in forty-five minutes, the longeſt duration of any of the experiments, it had gained three degrees. As this acceſſion was regular, if it had been greater it would not have invalidated the foregoing obſervations.

Many other trials were made on the effects of immerſion in water on the human heat, which I ſhall ſpeak of generally, under the general concluſions which they ſuggeſted.

The experiments already recited, ſuggeſted to me the notion, that in all changes from one medium to another of different denſity, though of the ſame temperature, there is a loſs of animal heat. I found, however, that this concluſion requires many reſtrictions.

1. My experiments being made on bodies of ſuch very different denſity as air and water, do not admit an univerſal inference of this ſort.

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* The author does not mean to object to the theories of Dr. Crawford and Dr. Goodwin. The decompoſition of the atmoſphere in reſpiration, is in all probability the principal ſource by which caloric is received into the ſyſtem, but the actual ſtate of heat in the body cannot ſafely be inferred (as ſome have ſuppoſed) from the quantity of air reſpired. Caloric is received into the ſyſtem by other proceſſes alſo, particularly from the food and drink in the ſtate of digeſtion, and the evolution of it into active heat appears to be modified by the living power on its own peculiar principles, and cannot be explained by chemical or mechanical principles or analogies.

2. Being all made in a temperature fifty degrees under the human heat, no certain concluſion can be drawn as to what might happen in degrees of heat much higher, where it is probable, the effects of the change, if it appeared at all, might be leſs ſtriking. It would ſeem, however, that after a perſon is long chilled in cold water, the firſt effect of paſſing through the external air into the warm bath, is firſt a fall of heat in the air, and after this a ſtill greater fall in the warm bath, followed, however, by a ſpeedy riſe.

The air and the water being equally cold, and both 45° or under, I found the loſs of heat in paſſing from the one to the other to be regulated in the following way.

1. If, inſtead of being expoſed naked to the wind previous to immerſion in the water, the body was kept warm by flannel covering, the mercury fell much leſs on the firſt plunge.

2. If, after plunging in the water, the perſon continued in it only a minute or two, a ſubſequent fall of the mercury did not always take place, on his emerging into the air. On the contrary there was ſometimes a riſe on ſuch occaſions in the mercury, eſpecially if the atmoſphere was at reſt.

3. In one inſtance, after continuing in the water fifteen minutes, on riſing into the air in a perfect calm, though during a froſt, there was little or no ſeeming diminution of the heat; while expoſure under ſimilar circumſtances, with a north-eaſt wind blowing ſharply, though the air was many degrees warmer, produced a rapid diminution. The effects of the wind in diminiſhing the human heat are indeed ſtriking, and are not in my opinion explained by the common ſuppoſitions.

4. The loſs of heat by a change of media, depends much on the rapidity of the change, for the plaſtic power of *life* in varying the proceſs of animal heat, ſo as to accommodate it to the external changes, acts for a time with great celerity, though this celerity ſeems to diminiſh with the ſtrength.

EXPERIMENT VIII.

I placed in a large room, where the mercury ſtood at 36° two ſlipper baths at the diſtance of ſix yards from each other. One was filled with cold ſalt-water of the temperature of 36°, the other with water heated to 96°, which was my own heat. Undreſſing myſelf in an adjoining room by a fire, I afterwards ſlipped on a looſe flannel dreſs, and deſcended *ſlowly* into the cold bath, where I remained two minutes; I aſcended *ſlowly* into the air, and then ſunk myſelf in the warm bath, where I remained two minutes alſo: I returned to the cold bath, where I ſtaid two minutes as before, and removed from it again to the warm bath. But during all theſe changes of media and temperature, the thermometer with its bulb under my tongue never varied from 96°. I attribute this partly to the heat of my body being in ſome degree defended by the flannel dreſs, partly to the calm of the air, but chiefly to the ſlowneſs of motion in theſe changes. It may be ſaid that the time of ſtaying in the different baths was not long enough to produce any ſenſible change in the heat of circulating fluids of ſuch a maſs, but this is not conſiſtent with many of the other facts.

5. The influence of the application of cold water to the ſurface of the body on the heat, is in ſome reſpects regulated by the animal vigour, as the following experiment will ſhow.

EXPERIMENT IX.

In the ſame room I placed a large empty veſſel, in which two young men ſat down in ſucceſſion, each with the bulb of a thermometer under his tongue. A man ſtanding on a bench with a bucket of cold ſalt-water containing four gallons, poured the whole on the head and ſhoulders, ſuffering it to run down on the reſt of the body. This proceſs took up nearly a minute, during which I examined the mercury, and found it unchanged. They were both directed to continue ſitting without motion for a minute after, during which, in both inſtances, the mercury roſe two degrees. A third, much inferior in vigour, ſubmitted to the ſame experiment, and the mercury continued during the affuſion of the water unchanged, but in a minute after ſunk half a degree. In fevers where the heat is generally increaſed from two to ſix degrees above the ſtandard of health, pouring a bucket of cold water on the head always reduces the pulſe in frequency, and commonly lowers the heat from two to four or five degrees. Of this ſalutary practice I hope ſoon to ſpeak at large to the public

6. The power of the body in preſerving its heat under the impreſſions of cold, and the changes of temperature, and of media, ſeems in ſome meaſure regulated by the condition of the mind. That fear increaſes the influence of cold, and of many other noxious powers, will not be doubted; but the ſtate of the mind to which I allude, is that of *vigorous attention* to other objects. This, it is well known, will to a certain degree deaden, or, indeed, prevent the ſenſation of cold; and what does this, I apprehend, prevents, or at leaſt weakens, its phyſical action. The aſtronomer, intent on the objects of his ſublime ſcience, it is ſaid, neither feels, nor is injured by, the damps or the chillneſs of the night; and in ſome ſpecies of madneſs, where the ideas

of imagination are too vivid to admit the impreſſions of ſenſe, cold is reſiſted to an extraordinary degree. I have ſeen a young woman, once of the greateſt delicacy of frame, ſtruck with madneſs, lie all night on a cold floor, with hardly the covering that decency requires, when the water was frozen on the table by her, and the milk that ſhe was to feed on was a maſs of ice.

7th. There are particular conditions of the atmoſphere, not perfectly underſtood, that ſeem to have an influence in depriving us more ſpeedily of our animal heat, than others where the cold is greater.

It may ſeem that by this time I had renounced my intention of trying the effects of immerſion in freſh water on the animal powers, and particularly on the heat. Some trials, I have, however, made, of which I ſhall only relate the following.

EXPERIMENT X.

In the ſame veſſel containing an equal bulk of freſh water, Richard Edwards, the ſubject of my firſt experiments, was immerſed, at the ſame hour of the day. His heat previouſly was 98°, his pulſe beat 92 in the minute: the heat in the air was 41°½, that of the water 40°. The wind was now in the weſt, ſo that in the court where the bath ſtood there was a perfect calm. As I had ſome fears of the iſſue of this experiment, inſtead of expoſing him for a minute naked to the wind before immerſion, he was covered with a flannel dreſs from the air, till the inſtant he deſcended into the water, into which he was ſuffered to ſink himſelf ſlowly, with the bulb of the thermometer under his tongue. Theſe are important circumſtances. The following table exhibits the reſult.

Immediately

Immediately on immerſion, heat	98°	14 min. after, heat	96° ½
1 minute after	97 ½	15	96
2 minutes	97	16, 17, 18, 19, 20	96
3 ——	98	21, 22, 23, 24	
4 ——	97 ½	25 ——	95
5 ——	96	26 ——	94
6 ——	96	27 ——	93 ½
7, 8	96	28, 29	94
9 ——	97	30	93
10 ——	97	31, 32	94
11, 12, 13		33, 34	92 ½

He now got out into the air very ſlowly, and ſtood in it three minutes, the wind *not* blowing on him. He loſt one degree of heat at firſt, which he recovered. He was then put into a warm bath at 90°, which at firſt *he felt* warm, and his feet and hands were pained: but in two minutes he fell into a very violent ſhiver, and his heat fell two degrees. The bath was then heated to 95 and 96°, but ſtill he felt cold. It was heated to 99°: he continued in it five minutes and his heat was 91°. The heat was gradually raiſed to 106°, when the ſenſe of coldneſs of which he had complained at the pit of the ſtomach gradually went off. Before this I had uſually kept him in the warm bath till his natural heat was nearly recovered, but after being half an hour in the heat of 106°, his own heat was ſtill 93°. He now became ſick and very languid, a cold ſweat covering his face, his pulſe very quick and feeble He was removed into bed, but paſſed a feveriſh night, and next day had wandering pains over his body, with great debility, reſembling the beginning ſtage of a fever. By cordials and reſt this went off.

This experiment clearly enough confirms the greater danger of being wet with freſh than ſalt-water ; but in itſelf points out nothing certain beſides, except that it is not to be raſhly repeated. I mean to try ſome of theſe experiments to a greater extent on the brute creation, when I have procured thermometers better ſuited to my views. The thermometers I employed had not a ſufficient mobility for very nice experiments, and I am well aware, that in particular inſtances this may have miſled me, though the general reſults, which is all that is of importance in ſuch experiments as theſe, will I hope be found juſt and true.

Before I conclude, I muſt offer a few obſervations on the ſubject that led to theſe experiments.

1. It is, I think, already well known among ſeamen, that where there is only the choice of being wet with ſalt or freſh water, it is always ſafeſt to prefer the firſt. In the heavy ſhowers of rain, hail, or ſnow, by which gales of wind are generally accompanied, the men that muſt be expoſed to them, ought, like Lieutenant Bligh and his crew, to wring their clothes out of ſalt-water.

2 In all caſes where men are reduced to ſuch diſtreſs by ſhipwreck or otherwiſe, that they have it only in their power to chuſe between keeping the limbs conſtantly immerged in the ſea, or of expoſing them to thea ir while it rains or ſnows, or of being expoſed to it, where the ſea is at times waſhing over them, it is ſafeſt to prefer a conſtant immerſion ; becauſe, in the northern regions, where the cold becomes dangerous to life, the ſea is almoſt always warmer than the air, as the experiments of Sir Charles Douglas ſhew ; and becauſe there is not only a danger from the in-

creaſed cold produced by evaporation, but alſo from the loſs of heat by the rapid changes of the ſurrounding medium, as the foregoing experiments point out.

3. Whether, in high and cold winds without rain or ſnow, and where a ſituation may be choſen beyond the reach of the waves, it is ſafer to continue in the air, or to ſeek refuge in the ſea, muſt depend upon ſeveral circumſtances, and cannot perhaps be certainly determined. The motives for chooſing the ſea will be ſtronger in proportion as the wind is high and cold, and in proportion as the ſhore is bold.

The foregoing narrative ſhows that men may ſurvive twenty-three hours immerſion in the ſea, of the temperature of 38° or 40° (as great a cold as it almoſt ever poſſeſſes) without food or water, and almoſt without hope of relief; but that any man ever ſurvived an equally long expoſure to the higher degrees of cold of the atmoſphere, in the ſame circumſtances, does not appear. Though in the caſe related, immerſion in water did not prevent thirſt, yet there is no doubt that it alleviated it, a circumſtance of high importance towards the preſervation of life.

Liverpool,
December 25, 1791.

P. S. I have purpoſely avoided any reaſoning on the cauſes of the loſs of vital heat on the change of media in the experiments recited. It may be ſuppoſed that during immerſion, the water immediately in contact with the ſkin, having become heated to a certain degree, the naked body, on riſing from it into the air, was in fact expoſed to a colder medium, and thus the loſs of heat, in this inſtance, produced.

My examination of the heat of the water during immerſion, not having been made in contact with the body, I will not deny that there is ſome foundation for the remark; and the caſes, it muſt be allowed, are by no means exactly parallel between immerſion in an open veſſel, however large, and immerſion in the ſea, where the conſtant undulation may be preſumed to occaſion a continual change in the ſurrounding fluid. But whatever allowance may be made for the circumſtance mentioned, I am perſuaded that the difference between the denſity of air and water being conſidered, it is not ſufficient to explain the loſs of heat in the inſtance alluded to. The changes of temperature in the living body are governed by laws peculiar to itſelf. I have found, in certain diſeaſes, greater and ſuddener variations than any mentioned, from applications of cold very gentle in degree, and momentary in duration.

In his maſterly "Experiments and Obſervations on Animal Heat," Mr. Hunter has objected to taking the heat of the human body by introducing the bulb of the thermometer into the mouth, becauſe it may be affected by the cold air in breathing. The objection is well founded if the bulb be placed on the upper ſurface of the tongue, but if it be under it and the lips ſhut, the effects of reſpiration may be diſregarded, as I have found from many hundred experiments. The heat may be obſerved in this way with eaſe and certainty, by employing thermometers curved at that end to which the bulb is affixed (the bulb being introduced at the corner of the mouth,) ſome of which have been made for me by Mr. Ramſden, according to a form given, as well as others on Mr. Hunter's plan. From repeated trials it appears to me, that when the uſual clothing is on, the heat of the living body may be taken with nearly the ſame reſult and equal certainty, under the tongue with the lips ſhut, at the axilla with the arm cloſe to the ſide, and in the hollow between the ſcrotum and the thigh ; every other part of the ſurface is liable to variation and uncertainty. It is evident, that of theſe three methods, the firſt only can be employed (as far as I can diſcover) when the trunk of the body is immerſed in water ; and even when the naked body is expoſed to the cold air, the firſt method ſeems the beſt, the heat remaining moſt ſteady under the tongue ; the axilla is the next beſt in order, and the worſt, the lower part of the groin ; for the ſcrotum and the parts of generation loſe their heat on the application of cold, more ſpeedily perhaps than any other part of the body, the extremities not excepted.

N. B. The water employed in the experiments related, contained ſalt in the proportion of one to twenty-four.